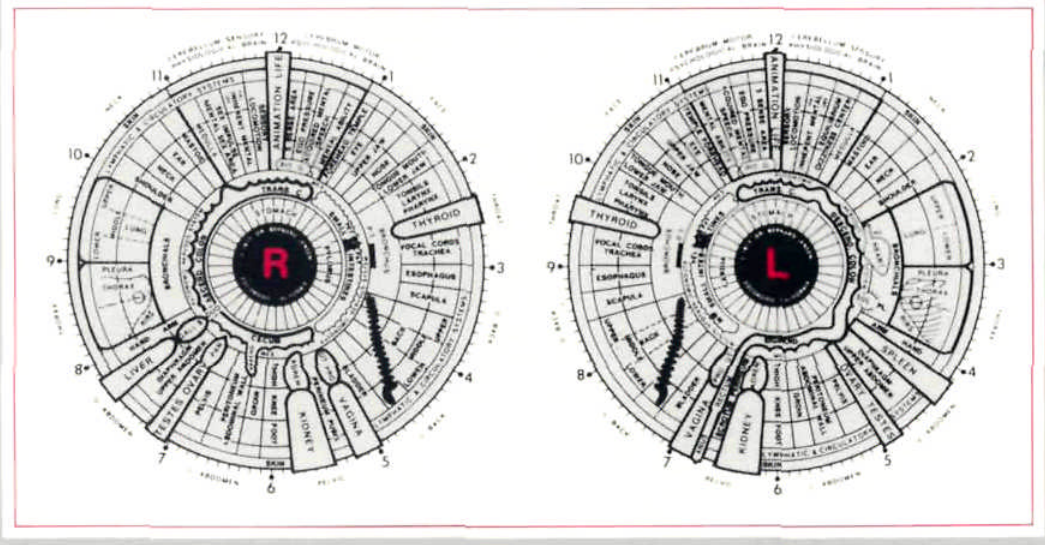
the Skeptical Inquirer

Iridology: Diagnosis or Delusion?



How the Nazca Drawings Were Done

Pseudoscience and the University

Numerology / Psychics / Paluxy Footprints

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the Skeptical Inquirer

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ON THE COVER: Iridology chart developed by Dr. Bernard Jensen, D.C. Copyright © 1980 by Dr. Bernard Jensen, D.C., Escondido.

News and Comment

Raiders of the Lost Tracks: The Best Little Footprints in Texas

Creationists have written about ten books and dozens of articles on the alleged co-occurrence of human and dinosaur tracks in the 100-million-year-old Cretaceous limestone of the Paluxy River bed near Glen Rose, Texas. All of the major books promoting "scientific creationism" mention the Paluxy River "man tracks" as evidence that the geologic time-scale is wrong and that therefore the evidence for evolution is not as good as scientists think.

Scientists currently think that the dinosaurs became extinct 65 million years before the appearance of the genus *Homo*. But creationists, in their lectures and debates before public audiences, almost invariably present the contemporarity of humans and dinosaurs as fact. Because the presence together of "human" and dinosaur footprints is so widely touted by creationists in their writings and speeches for the public, and also because a recent excavation by two Baptist ministers at a site on the Paluxy River resulted in the discovery of new "human" footprints and received wide exposure in the national press, a number of scientists decided it was time for a thorough on-site examination of the elongated depressions that the creationists claimed are human footprints. The examination was both appropriate and timely: although the creationists have promoted the human tracks for twenty years, the scientific response to their claims has been either to ridicule or to ignore them, and scientists have now learned that this response is counterproductive. Therefore, anthropologists Laurie Godfrey and John Cole, geologist and paleontologist Steven Schafersman, and science teacher Ronnie Hastings journeyed to Glen Rose to examine the footprints for first-hand information to explain why the "human tracks" are not human footprints at all, and what actually caused them.

The limestone bed of the Paluxy River has an international reputation for the excellence and quantity of the dinosaur footprints found there. Vertebrate paleontologists and ichnologists (trace fossil experts) have studied the footprints for years, and Dinosaur Valley State Park was created to preserve them and make them accessible to the public. The few scientists who have professionally studied the dinosaur footprints have been the primary source of information for the press on the rare occasions when a rebuttal to the creationists' claims was desired. The scientists' explanations about the real nature of the features the creationists were confusing with legitimate human footprints were quite accurate, but they did not get the

attention they deserved. They were always presented to the public in a context of disagreeing with the pseudoscientific claims of the creationists. There was, and is, no single place that one could turn to obtain the necessary information to refute the creationists about this particular problem.

To avoid this mistake in the future, Godfrey and her colleagues plan to publish a detailed and illustrated monograph on the "human footprints," including their history and place in the culture of pseudoscience, in a future issue of *Creation/Evolution*. Readers of the SKEPTICAL INQUIRER are aware that scientists are now directly answering creationist assertions by explaining, in numerous books and popular articles, how science works, why evolution is a factual scientific concept, and how the creationists have distorted and misrepresented the scientific evidence in their campaign to deceive the public. An excellent example of this new activity among scientists is the book *Scientists Confront Creationism*, edited by Laurie Godfrey and containing papers by herself, Cole, Schafersman, and others; the book will be available in early 1983.

The results of the Paluxy River expedition are easy to summarize; no evidence for genuine human footprints was found among all the tracks examined, but the scientific investigators were able to discover six major methods by which the elongate depressions, which the creationists claim to be of human origin, were formed. The investigators examined, measured, mapped, and photographed all the specific objects that are identified as human footprints in the creationist literature. Three sites were investigated in detail: the State Park Ledge, the McFall site (where the excavation of International Baptist College is in progress), and a site near New Braunfels, Texas.

Steven Schafersman

Godfrey and Cole measuring depression in Cretaceous limestone on banks of Paluxy River. Three dinosaur footprints are in foreground.

Not a single "man track" examined had the correct anatomic features of a human foot either at rest or in stride. This observation is also true for the famous carvings of the giant man tracks from Glen Rose. The person who carved these tracks had only a primitive knowledge of human foot anatomy and produced tracks that superficially resembled giant human footprints but that are quite unmistakably artifacts to a trained physical anthropologist. These notorious carvings are not accepted as real human footprints by some of today's sophisticated creationists, but most believe they are genuine. Most older creationist books, such as Whitcomb and Morris's *The Genesis Flood*, illustrate these carved tracks and assert that they are real. None of these tracks, however, have proper human footprint dimensions or impressions of the instep, toes, or ball of the foot. The "human track trail" we examined had stride and pace lengths and directions uncharacteristic of

human beings. Most of the creationist-identified human footprints are isolated from others but are adjacent to well-preserved dinosaur tracks. This fact assumes great importance in our discussion of how the "human" tracks were formed.

If we ignore these carvings, the origin of all the oblong or elongate depressions in the Cretaceous limestone that the creationists claim are human footprints can be explained by natural,

Fossil Footprints in Stone

I have been somewhat amused by the foolishness of creationists and other misguided people who claim that human footprints in stone have been found side by side with dinosaur footprints. Exactly what this has to do with the advancement of religious beliefs sometimes escapes me. In any event, the claim is not to be taken seriously.

Sedimentary rocks, dating back before, during, and after the heyday of the dinosaurs, exhibit a variety of primary features. These are features formed at the same time as the sediment. Thus we can see ripple marks, raindrop imprints, cross-bedding, and tracks and trails of a variety of animals. For years these have been of great value to geologists in reconstructing ancient environments. It is not surprising that some of the myriad impressions made into soft sediment through millions of years might resemble a human footprint. In fact, some bipedal dinosaurs, when running, made imprints suggestive of human imprints. We know this because the same set of tracks, after the dinosaur slowed down, showed an unmistakable dinosaur. This can be seen in tracks made in Upper Triassic shales in the eastern United States.

I recall that several years ago in west Texas a rancher told me that the buffalo roamed more than 200 million years ago in west Texas and that he had the evidence to prove it. He took me to a place on his ranch where, indeed, the footprints of a buffalo were indented into Triassic age sandstones. I had been studying and mapping these sandstones for more than two years. What was the cause of buffalo footprints in rocks 200 million years old? The line of tracks led to a waterhole. Over many decades, buffalo trafficked to the waterhole following exactly the tracks of the leading bull, a well observed instinct. Triassic sandstone, despite its antiquity, is soft—soft enough in places to crumble in your hand. So, well-worn tracks emplaced within the past few centuries in 200-million-year-old rock would be enthusiastically acclaimed by the creationists as proof that the buffalo existed 200 million years ago in the same way that they claim humans existed along with the dinosaurs. They didn't. This is the stuff on which false legends are built.

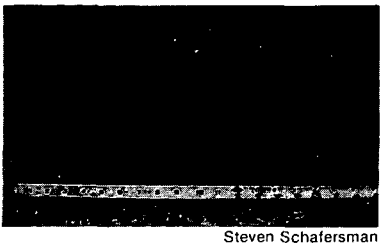
—*Charles J. Cazeau*

Charles Cazeau is an associate professor of geology at SUNY-Buffalo and coauthor of Exploring the Unknown.

nonhuman causes. Although this statement has been made by scientists for more than twenty years, the different "human tracks" have different origins, and it is now possible, after detailed investigation by the team of scientists, to ascribe the origin of each specific impression to a specific cause.

Most of the "man tracks" are simple erosion channels or gouges on the limestone river banks produced by water currents during flood conditions. Rapidly moving water is a powerful erosive force that can produce an endless variety of erosional features in hard limestone. One would expect to find a few superficially human-foot-shaped depressions in the Paluxy River banks, and most of the "man tracks" turn out to be these. Most of the State Park Ledge site "human footprints," the ones most people see, are simple erosional features. The tracklike depressions are invariably elongated parallel to the river and occur together with depressions of similar size and depth but different shape. The "insteps" of these erosional "footprints" are created by the undercutting of limestone layers of different hardness by the water current.

Since river erosion is such an easily understood and logical explanation for the oblong depressions, many persons who hear this explanation are quick to dismiss the claims of the creationists. Therefore, the creationists have gone to a great deal of effort to remove a thick layer of limestone at the McFall site to expose a limestone bed underneath that contains many well-preserved dinosaur tracks and, in the words of the project's director, the Reverend Carl Baugh, "twenty-nine human tracks, 27 of which are 16 inches in length." The creationists are pleased with these tracks, since there is obviously no way their existence can be ascribed to erosion by flood water. But what is one supposed to think of 16-inch-long featureless depressions that are all about 3 or 4 inches wide? The creationists believe that these are the tracks of giant men with odd-shaped feet (Genesis, after all, says only that "in those days, giants walked the earth"; the book is silent about the shape of their feet). These elongate depressions have no anatomic features that suggest to a scientist that they are of human origin. Yet the question of what caused them remains. There are several reasonable, natural explanations. The depressions could be primary sedimentary structures, such as channel, gouge, or flute marks, formed during erosion of the lime mud tidal flat before it was buried by later sediment and subsequently lithified. The depressions could be *karren* features formed by leaching (dissolving) the limestone by rain water, which seeps into the rock along joints and fractures and produces elongate cavities in the soluble limestone. Both of these processes are excellent natural explanations for the origin of oblong or elongate depressions.



Steven Schafersman

Footprint set into ground one block from Glen Rose, Texas, courthouse said to be a giant man-track by the local fundamentalists but is obviously carved.

The best and probably correct explanation, however, is that dinosaurs produced these features. The elongate depressions that the Reverend Baugh and his creationist colleagues claim to be human footprints are almost always found associated with three-toed dinosaur tracks, are the same depth as the

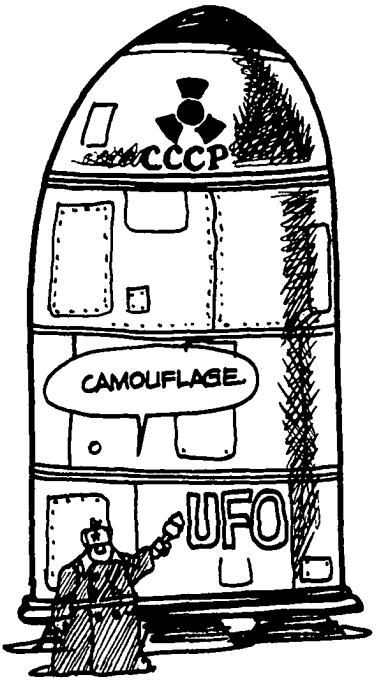
dinosaur tracks, are the same length as the dinosaur tracks (16 inches), and sometimes occur in trails of the same pace-length as dinosaur tracks. A genuine human footprint in the limestone would be much shorter and shallower than the observed depressions. Sometimes the two outer toes of a three-toed dinosaur track are indistinct or absent; this is caused by the dinosaur putting most pressure on the central toe, by the tidal flat mud being so firm that only the central toe produced an impression, and by water scouring the tidal flat and eroding the shallower outer toe prints before the impression hardened. Such an odd-shaped or distorted dinosaur track would look like an elongate depression of approximately the same dimensions as those seen at the McFall site. Another mechanism involves a dinosaur putting its foot onto a part of the mud flat that is extremely soft. Its foot sinks in and, upon withdrawing it, the mud flows back in the hole from the sides. The resulting oblong depression would be exactly the same length and approximately the same depth as the adjacent dinosaur tracks, but would be narrower and show no toes. Since this mechanism produces features essentially identical to those observed, it is the preferred explanation.

The elongate depressions could also possibly be caused by the rear toe on the dinosaur foot dragging a channel in the mud or by insertion of the foot sideways into the mud. One "man track" at the McFall site was created by the trace fossil *Planolites*, which was extremely abundant on the limestone surface. The burrow casts of *Planolites* formed ridges between five perfectly spaced "toes." At the New Braunfels site, creationist-identified "man tracks" are actually dinosaur tracks distorted by repeated growth of a Cretaceous algal mat in and around each footprint depression on the tidal flat. After several such layers have accumulated, the dinosaur prints are indistinct, and some assume a roughly oblong shape; creationists claim these are human despite the fact that they occur in perfect spacing and orientation with the well-preserved and exposed dinosaur track trails. The point is that there are many natural explanations that need to be considered before resorting to fantastic explanations for elongate depressions in Cretaceous limestone—especially explanations that conflict with much of what we know about evolutionary history and stratigraphy. It is fair to say that the investigation reported here has falsified the claims of the creationists and discredited their attempt to present themselves as legitimate scientists.

—Steven Schafersman

Steven Schafersman is a geologist and president of the Texas Council for Science Education.

Soviet UFOs, Missiles, and the Gindilis Report



More than fifteen years ago, in 1967, a wave of UFO sightings over the Soviet Union provoked widespread comment and publicity. The February 1968 *Soviet Life* carried a graphic description of one of the apparitions: "It was shaped like an asymmetrical crescent . . . Narrow, faintly luminous ribbons . . . followed behind. . . . The object was flying horizontally in the northern part of the sky, from west to east, at about twenty degrees above the horizon. . . . As it moved away from the observers, the crescent dwindled, turned into a small disk, and then suddenly vanished."

Soviet UFO "expert" Felix Zigel called it one of the best unsolved UFO apparitions on record.

Western UFO books have referred to the sightings as "giant spaceships," or flying saucers hundreds of yards in diameter. In his 1973 book *Aliens from Space*, Donald Keyhoe described the twilight scene in July 1967: "Suddenly a huge flying object appeared, moving swiftly across the sky . . . It was an amazing sight—an enormous crescent-shaped craft at least eight times larger than any known airplane. The horns of the crescent were pointed backward, emitting jetlike exhausts." He said it was flanked by "smaller UFOs, which kept precise formations, matching the crescent's terrific speeds."

Houston space engineer and Soviet space-program expert James E. Oberg has now identified the cause of this spectacular wave of Soviet UFOs. Oberg says the UFO phenomena were caused by test flights of a Soviet military vehicle, the Fractional Orbit Bombardment System (FOBS). The FOBS system was designed to be an orbital H-bomb carrier best suited for a nuclear sneak attack. He says that, although Moscow signed the treaty outlawing placement of H-bombs in orbit later in 1967, in fact it continued to test FOBS vehicles (now outlawed by international law) long afterward.

The FOBS test flights involved a single loop around the earth, a temporary achievement of orbit, then a flaming plunge back into the atmosphere. The three most spectacular multiple-witness reports in the 1967 UFO "wave" were on July 17, September 19, and October 18. Oberg's check of space-vehicle launch records found that the times and flight paths of the fiery FOBS-test re-entries coincided nicely with the reported times of the three mass sightings of UFOs. For instance, on September 19 a series of UFO sightings were reported across the southern USSR from 7:20 to 8:20 P.M., along a west-to-east path. Meanwhile, says Oberg, a Soviet FOBS missile had blasted off from Tyuratam in Kazakhstan shortly before 6 P.M., circled the

planet, and was "flaming its way across the southern Soviet skies at 7:30."

A much-hailed 1979 official report on UFOs from the USSR Academy of Sciences, known as the Gindilis report, unwittingly supplied information for the correlations. According to Oberg, for the May-October 1967 period, there were eight FOBS flights, and seven of them appeared in the table of 1967 UFOs in the Gindilis report. The report lists 56 multiple-witness cases in that time period; 44 of them correlate to the dates of FOBS flights.

"Almost 80 percent of the UFO sightings of the period of interest in 1967 were evidently caused by FOBS space missions," says Oberg. The identification not only solves a series of dramatic UFO reports but also casts serious doubt on the credibility of both the Gindilis report, which concluded that UFOs are "real" in a mathematically provable sense, and Western UFO experts, who enthusiastically endorsed it and misrepresented it as dealing only with "true UFOs."

As Oberg says: "A full three-quarters of the total number of UFO reports analyzed by the Gindilis report were from 1967! So the official Soviet statistical study's results are hopelessly polluted by non-UFO data (that is, the FOBS sightings) and hence are totally worthless as information about 'true UFOs' and their reputed 'stable statistical properties'—which the authors and Western reviewers boasted about."

Concludes Oberg: "Computer experts have a saying: 'Garbage In, Garbage Out.' The Gindilis report by this definition is garbage, and a lot of UFOlogists eagerly swallowed that garbage. It should leave a bitter taste in their mouths."

Oberg suggests that the Gindilis report is a ruse, designed to divert attention from the real cause of the "UFOs"—secret Soviet missile tests and satellite launches. Oberg has written a two-part account of his conclusions for the MUFON *UFO Journal* (November-December 1982).

—*Kendrick Frazier*

Psychics' 1982 Predictions: Our Annual Look Backward

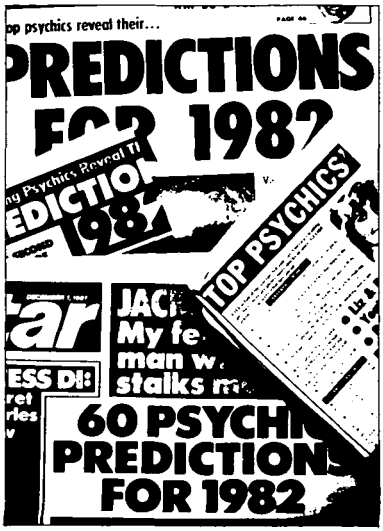
And now, folks, it's time for that annual tradition, a look at psychics' predictions for the year. Not for the current year, 1983, but, as is our infinitely more enlightening practice, for last year, 1982.

The relevant scholarly journals consulted are the supermarket tabloids the *Star*, the *Weekly World News*, the *Globe*, and the *National Enquirer*. Here are some of the prognostications psychics made for 1982.

From "Predictions for 1982," *Weekly World News*, December 1, 1981:

- "A secret document will slip through the iron curtain revealing that the Soviet Union [has] established contacts with beings from another planet."
- "Startling new evidence of pyramids and massive statues on Mars will convince scientists that life flourished there millions of years ago."
- "A 'war of the worlds' alarm will be sounded in the early summer when a fleet of UFOs buzz a top-secret military base in the Southwest. Surface-to-air missiles will be fired."
- "A sensational scandal involving money from casinos and organized crime will rock Monaco and even threaten Prince Rainier's crown." (No mention of Princess Grace's tragic death.)
- "Cuban President Fidel Castro is overthrown in a major uprising."

From "Sixty Psychic Predictions



for 1982," *Star*, December 1, 1981:

- "Chrysler Corporation will be unable to meet its obligations and will go out of business." (Although formerly in deep trouble, Chrysler announced a profitable 1982 third quarter.)
- Linda Ronstadt and California Governor Jerry Brown "will marry in 1982."
- "Scientists will discover life in outer space through satellites that are already in space."
- "The latter part of 1982 will see gasoline supplies getting tighter and prices rising still higher." (Prices dropped all through the second half of 1982.)
- "On the economic front, 1982 will be one of the worst years for the U.S. since 1929. The stock market will crash." (The New York Stock Exchange index reached its highest level ever.)
- "A UFO will cause a catastrophe in the air in South America during the summer."

From "Jeane Dixon Predictions," *Star*, June 29, 1982:

- "Two beautiful women from Frank Sinatra's past are destined to play important roles in his future. The first seems to be Sophia Loren. . . . The second lady from the past could be Grace Kelly, Princess of Monaco. This reunion seems to be coming next year [1983] and could result in an important new film for the High Society co-stars."
- "Sugar Ray Leonard will fight again." (He retired.)
- "An impeachment effort will be launched against one or more Supreme Court justices."

From "Top Psychics' Predictions," *Globe*, June 22, 1982:

- "A UFO will land in Washington, D.C.—right beside the White House."
- "A controversy will break out when it's discovered that a man-made explosive device sank the Titanic."

From "Ten Leading Psychics Reveal Their Predictions for the Second Half of 1982," *National Enquirer*, July 6, 1982:

- "A new messiah will emerge in El Salvador, a 9-year-old boy who'll perform miracles and use his vast influence to halt the bloodshed in that country."
- "Ex-astronauts Neil Armstrong and Buzz Aldrin will blow the lid off a UFO cover-up by the government. They'll reveal the existence of photos of alien spacecraft and of two aliens walking in space." (Actually, ex-astronaut Charles Conrad, on the NOVA TV program on UFOs in October, said promulgators of astronaut UFO stories were "full of beans.")

Finally, we cheerfully offer our own prediction. When they recount their record for 1982 in announcing their predictions for 1983 and for 1984, the "psychics" will offer nothing but successes. All of the prognostications above will have conveniently vanished from their "psychic" memories.

—*K.F.*

Random Notes on the Psi Scene

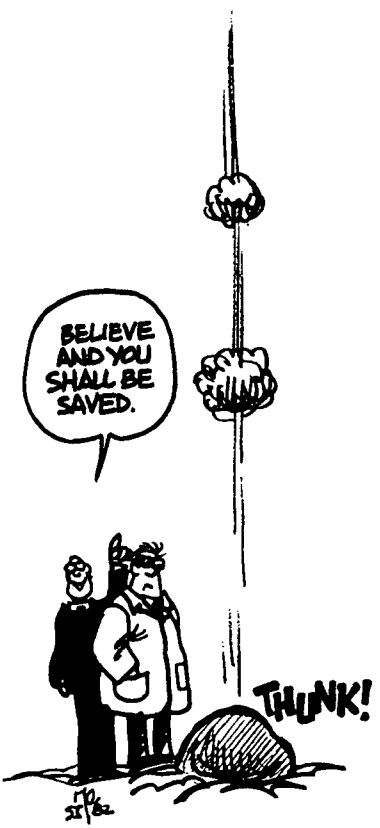
Defenders of the paranormal are fond of pointing out how scientists once refused to believe that meteorites were stones that fell out of the sky. It's so obvious, they say, yet science disbelieved. To them that confirms their view that scientists are closed-minded about controversial issues, such as paranormal claims.

One of the strongest proponents of this view is sociologist Ron Westrum of Eastern Michigan University. He gave a paper on the subject at the first meeting of the Society for Scientific Exploration last summer.

Meteorite expert Roy S. Clarke, Jr., of the Smithsonian Institution complains that although Westrum's facts are right his hind-sighted interpretations leave something to be desired.

In a letter published in the September 3 issue of *Science*, Clarke criticizes Westrum for producing an indictment instead of an insightful analysis: "Westrum does not acknowledge that the individuals who worried about the reality of meteorite falls at the end of the eighteenth century faced a difficult intellectual challenge. The development of a conceptual framework for the recognition of meteorites within a period of two decades was a significant scientific achievement."

Concludes Clarke: "Those who participated should not be disparaged because the problems they faced 200 years ago can be made to appear trivial today."



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Leaders of nonfundamentalist religions continue to be among the strongest allies of scientists and educators in fighting the creationist movement. Their actions vividly emphasize that the dispute is not between science and religion.

The latest such example is an anti-creationist resolution adopted by the Sixty-seventh General Convention of the Episcopal Church last September in New Orleans.

The resolution's explanation summarizes the legislative efforts by creationists to pass laws requiring the teaching of "creation-science" wherever evolutionary models are taught. It then says: "The terms 'Creationism' and 'Creation-science' as understood in these laws do not refer simply to the affirmation that God created the Earth and heavens and everything in them, but specify certain methods and timing

of the creative acts, and impose limits on these acts which are neither scriptural nor accepted by many Christians. The dogma of 'Creationism' and 'Creation-science' as understood in the above contexts has been discredited by scientific and theological studies and rejected in the statements of many church leaders."

The text of the resolution:

> *Resolved*, the House of Bishops concurring, That this 67th General Convention affirm its belief in the glorious ability of God to create in any manner, and in this affirmation reject the rigid dogmatism of the 'Creationist' movement; and be it further
>
> *Resolved*, That we affirm our support of the scientists, educators, and theologians in the search for truth in this Creation that God has given and entrusted us.

* * * * *

When "The Case of the UFOs" was shown in London (BBC produced it) in October, it brought a very favorable review in the *Times* of London. The review strongly praised the program's resolute skepticism, revealing "a powerful UFO mythology." But, strangely, the review ended by referring to alleged cases of spontaneous human combustion, "when an individual literally bursts into flames." The reviewer apparently takes such reports seriously. He wrote that the cases suggest that "human beings can project energy outwards from themselves . . . that such energy exists and can consume its carrier."

This prompted a letter of reply from, appropriately, a professor of combustion physics at the Imperial College of Science and Technology, Felix Weinberg. Weinberg challenged the reviewer to supply him with details documenting this ability to project energy and its supposed occasional

ALL I NEED IS THE YEAR, MONTH, DAY, TIME AND THE LOCATION OF YOUR BIRTH.
EQUAL OPPORTUNITY EMPLOYER
WE DO NOT DISCRIMINATE ON THE BASIS OF SEX, RELIGION, NATIONAL ORIGIN OR AGE... JUST SIGN.

consequence, bursting into flames oneself.

"Unfortunately," said the professor, "such excess of energy appears to be so rare an event in the depleted groves of academe (we are not talking about being fired with—or without—enthusiasm) that I have not come across a single instance of a colleague autocombusting, but I dare say it happens amongst journalists all the time."

* * * * *

One would think that activist consumer groups would be quick to condemn the consideration of a job-seeker's astrological sign in employment decisions. It is a blatant form of job discrimination. Yet just such a practice surfaced publicly recently. And it is going on in, of all places, Ralph Nader's Public Citizen organization.

According to a brief note titled "The Astrology Factor" in the *New York Times* (Oct. 14, 1982, p. B16) Joan Claybrook, most recently head of the National Highway Traffic Safety Administration and now back at Public Citizen after a four-year hiatus, likes to

consult the stars in interviewing job applicants.

"I'm not an astrologist," she was quoted as saying, "but I use astrology as a point of discussion in job interviews." For example, if an applicant is a Pisces, "I mention that one characteristic of this group is that they often go in two different directions, which leads to the question of whether they have this problem." When questioning a job-seeking Pisces, she says, she can't resist pointing out that Ralph Nader is one. "I ask them if their personal characteristics are similar to Ralph's, and they either become very positive or very defensive."

SUNY-Stony Brook political psychologist Ralph W. Bastedo, who wrote of job discrimination by astrological sign in his article on astrology in the Fall 1978 SKEPTICAL INQUIRER, says this is another example of "employment discrimination resulting from a misguided faith in a pseudoscience."

"Ralph Nader and Public Citizen should be cautioned on this matter," says Bastedo. "Discrimination on the basis of merit is one thing; hiring on the basis of one's feelings about zodiac signs is quite another."

* * * * *

Dialogue between the *SI* Editor and a customer at a photocopy center who noticed the SKEPTICAL INQUIRER letterhead:

"What is the 'Skeptical Inquirer'?"

"It's a publication that critiques fringe-science claims—UFOs, astrology, ESP, that kind of thing."

"Is there any interest in doing that?"

"Well, we've become quite well known and well regarded in scientific and philosophical circles."

"The people who gave us the atomic bomb."

"Who? [My mind searched for his meaning. Philosophers? Critics of the paranormal?]

"Scientists. They gave us the atomic bomb."

"They also gave us electricity."

"Well, that's debatable."

The customer stomps away. End of conversation.

* * * * *

Joe Nickell seems to have a passion for duplicating ancient and controversial artifacts to demonstrate how they could have been produced by quite natural and understandable (if clever) means. First it was the figure on the Shroud of Turin (see articles on the subject in *SI*, Spring 1982). Now, in this issue, he reports on the fulfillment of "a longtime goal." His article describes his use of only sticks and cord to design a full-scale duplication on a landfill in Kentucky of one of the larger drawings on Peru's Nazca Plain—a 440-foot condor. The work reveals how the Nazca Indians very probably laid out their intriguing giant ground-etchings. The method was simple, but the scale of Nickell's project required "heroic help" of friends and relatives. It also meant enduring some bouts with rain (seldom a problem at Nazca). But the effort and patience were well worth it, as the aerial photo of the 440-foot condor duplication—possibly the world's largest art reproduction —vividly shows. "To the best of my knowledge," says Nickell, "this has never been accomplished before, and I am excited by the results."

* * * * *

A 70-minute VHF videotape, "Footprints in the Mind," documenting the Paluxy River Expedition described on pages 3-6 of this issue, can be purchased for $20 from Dr. Ronnie Hastings, Science Department, Waxahachie High School, Waxachachie, TX 78715.

—Kendrick Frazier

CSICOP Council in Atlanta: Police Psychics, Local Groups

The Executive Council of CSICOP held its 1982 annual meeting in Atlanta December 9-10, and all members of the council were present: Paul Kurtz (chairman), George Abell, new member James E. Alcock, Kendrick Frazier, Martin Gardner, Ray Hyman, Philip J. Klass, Lee Nisbet, James Randi, and Marvin Zimmerman. A wide range of issues were discussed.

The meeting was dedicated to Martin Gardner. CSICOP presented him with an informal "In Praise of Reason" award "in honor of his heroic efforts in defense of reason and the dignity of the skeptical attitude."

The Atlanta location afforded an appropriate opportunity for a public post-mortem on the claims and predictions made by "psychic" Dorothy Allison during her internationally publicized visit to the city in October 1980 at the height of concern over the murders of black children.

CSICOP held a news conference on the subject of proclaimed "psychic detectives" and "psychics" in general on the morning of December 10. Seven media outlets, including the *Atlanta Journal and Constitution*, UPI, Atlanta's CBS-TV affiliate and a Swedish television crew attended.

Randi reported that Allison gave a total of 42 names to Atlanta police as her "psychic" contribution to the investigation. "None of them was 'Wayne' and none was 'Williams.'" (Wayne Williams was the name of the man eventually convicted in two of the slayings and implicated in many more.) "Yet Allison still claims that she was of help in the investigation," said Randi. "Nonsense." He characterized "police psychics" as people who "take advantage of the credulity of people" during times of tragedy and stress.

Kurtz, Frazier, Hyman, and Alcock also spoke at the news conference. Kurtz reviewed recent prominent incidents of psychic claims. He criticized the public's and the media's tendency to take such claims at face value and said members of the Committee investigate as many claims as they can and find that "they rest on tenuous grounds." Frazier called for greater media caution in reporting on psychic claims and recommended retrospective stories assessing the accuracy of them. Hyman emphasized that all of us, including some of our best minds, can be deceived by paranormal claimants. He said scientists in particular are easy prey to deceivers because their education and arrogance lend them "an illusion of invulnerability." As Hyman said, "Scientists are more prone to be taken in than other people because they think rationally. And once you know how a person's mind works, he can be taken in." Alcock noted that the "seeds of magical thinking" are in all of us and are especially prone to surface when we are faced with emotional issues or danger, such as when a murderer is running loose. "We should zealously guard the rational heritage that we have inherited from past generations."

In an article about the CSICOP news conference in the *Atlanta Journal/ Atlanta Constitution* the next day (December 11), Atlanta Commissioner of Public Safety George Napper, who was chief of police during the child murders, was quoted as saying he probably would not use psychics again.

* * * * *

The formation of local CSICOP-type groups was discussed at some length by the Council. Two such groups, the Bay Area Skeptics and the Austin Society to Stop Pseudoscience, have been in existence for more than a year. Inquiries have been received about forming similar groups elsewhere.

Most members felt such groups can be a potent force in advancing the causes of critical inquiry and public education about fringe-science and paranormal claims. Through regular meetings and person-to-person action they have the potential to respond more quickly and effectively to claims and issues at the local level than a national organization can. Council members felt the formation and activities of local groups should be encouraged.

The question at issue was what sort of relationship should exist between CSICOP and local groups. The consensus was that the relationship should be loose and informal. Possible guidelines for a policy were discussed. It was eventually decided to emphasize four points: local groups should generally agree with the aims of CSICOP; their members should speak only for themselves or their own group, not for CSICOP; their membership should include a "sufficient number" of people of scientific stature; and they should be autonomous. Local groups, it was decided, will be considered to be "autonomous groups functioning at the local level who share many of the goals of CSICOP."

* * * * *

CSICOP Chairman Paul Kurtz was authorized to begin planning a large, public meeting at which distinguished scientists and scholars would be invited to present papers on trends in the paranormal. The hope is that the meeting can be held at the SUNY-Buffalo campus in the fall of 1983. Contact Kurtz at CSICOP's Buffalo headquarters for later information.

* * * * *

The newest member of CSICOP's Executive Council is James E. Alcock, a professor of psychology at York University in Toronto. Alcock is author of *Parapsychology: Science or Magic?* (Pergamon, 1981). He is a CSICOP Fellow and is chairman of the Canadian CSICOP committee as well as co-chairman of CSICOP's Education Subcommittee. He has been a consulting editor to the SKEPTICAL INQUIRER since 1978.

—*K. F.*

Lonely UFO Vigil Ends in Death for Woman

Obsessed with UFOs and drawn by messages from "some higher power," Gerald Flach, 38, and Laverne Landis, 48, drove to the end of a trail in a remote area of the northwestern Minnesota wilderness. There, authorities said, they waited for nearly six weeks in their compact car, eating only vitamins and water.

On November 15, 1982, a motorist found Flach semiconscious on Gunflint Trail, 41 miles northwest of Grand Marais, Minnesota. Rescue workers found Landis dead in the front seat of the snowbound car on a small access road a few hundred feet from the main road. An autopsy showed she died of a combination of hypothermia, dehydration, and starvation.

Friends said Flach had become obsessed with UFOs in the preceding months, according to an Associated Press account of the tragedy. Cook County Deputy Sheriff Frank Redfield reported that Flach, who was now hospitalized in Grand Marais, had gone to the site as part of a UFO vigil. "Flach said he had been receiving messages through Ms. Landis from some higher power. The most recent message directed them to go to the end of the Gunflint Trail and await further mes-

sages. These people kind of believed in flying saucers."

Rescue worker Bruce Kerfoot said that from all appearances the two were "parked there like four to six weeks, just living in the front seat of their compact car. Flach told me he hadn't eaten in four weeks, and they both looked very, very gaunt."

Larry Hogan, a neighbor and longtime friend of Flach in West St. Paul, where Flach was an electrician, described him as a very intelligent, gentle man who had recently become obsessed with UFOs and space messages. "Gerry just changed his whole personality, his whole life in the past six to eight months," said Hogan. "He came on to this kind of obsession . . . purely with UFOs. I don't know why he did it."

A later, more detailed AP investigation of the case (published in December 12 newspapers) revealed that Flach and Landis were members of Search and Prove, a group whose members believe in contacting spirits through meditation and astral projection. A spirit named Mora purportedly spoke in a rasping voice "through" group leader Jerry Gross, advising members on life-style and the "seven astral planes of existence." The Search and Prove Study Center is in Afton, Minnesota.

Friends and relatives said Mrs. Landis, a registered nurse and a mother of five, was an extremely stable person but had a longstanding interest in psychic phenomena that approached an obsession after the death of her husband in 1975.

Flach and Landis had attended a psychic convention in California in early 1982. On May 17, they left their homes and families. During the months from May till October they drove through Minnesota, Wisconsin, and North Dakota seeking contact with spirits and UFOs. They reached the end of Gunflint Trail on October 5, guided there by a "contact" for a "pickup," Flach told Redfield. There they began their wait.

Sheriff's Deputy Kenneth Carlson, who interviewed Flach in the hospital, where he was under psychiatric treatment, said Flach told him he and Landis were "supposed to be picked up by a flying saucer . . . and get a transaction of some money . . . and make payments at Search and Prove and then get back with their families."

—*K.F.*

UFOs as a Future World Problem?

An estimated 100,000 children in the nation's schools have been studying "flying saucers" as part of a program whose stated objectives include "to encourage gifted children to begin looking for solutions to problems which they will encounter in the future" and "to enhance creative thinking and problem solving skills."

The Future Problem Solving Program, as it is called, is directed by Anne B. Crabbe, Coe College, Cedar Rapids, Iowa. The study of flying saucers was the first step in a multi-level competition that will culminate in a National Bowl at Coe College in late May. There the issue will be the possibility of nuclear war.

For $10, teachers and students could purchase from Coe College a "Resource Manual" to help them in local and state bowl competitions. For the initial competition dealing with Unidentified Flying Objects (UFOs), the manual suggested that students and their coaches consider such questions as:

- Why do (some) people absolutely refuse to entertain the possibility of visitors from other worlds, claiming

that all UFOs are caused by natural or man-made phenomena?

- UFO skeptics have pointed out that in none of the stories from UFO abductees have the ufonauts asked to meet with the leaders of Earth. Skeptics who believe this would be a first question use this as an argument to discredit the accounts. Can you think of reasons why no such request would be made by ufonauts?
- Many of the UFO reports include the stalling of automobile engines, changing brilliance of lights, interference on radios, and so on. If UFOs have the power to alter electrical and battery-operated devices, what effects, if any, could this pose to national security?
- What possible reasons could the government have for wanting to "hush up" the UFO phenomena?

The Future Problem Solving Program was created in 1974 by E. Paul Torrance as a project for Central High School in Athens, Georgia. In several years the program had expanded to other states. In 1981, Coe College offered to sponsor the program and hired Crabbe as its full-time director.

In the initial competition, students were asked to respond to the following: "Because large numbers of UFO sightings continue to be reported by responsible persons from all over the world, the United Nations has decided to become involved. Now, in the year 1990, the United Nations has asked you and your teammates for advice on what action the United Nations should take regarding UFOs."

The wisest response would be to recommend that the United Nations continue to ignore UFOs and to focus its efforts on the many pressing earthly problems. But it is doubtful that many contestants who aspired to enter the National Bowl reached this conclusion or submitted such advice, especially if the students, their coaches, and competition judges had purchased a Resource Manual and read its lengthy introduction, written by J. Allen Hynek (the nation's leading UFO promoter), and some of the books recommended in this manual.

For example, several books deal with persons who claimed to have been abducted by strange-looking creatures and taken aboard UFOs. One of these books contains an introduction by Hynek strongly endorsing it. Another recommended book claims that the U.S. Army captured a crashed saucer and its dead occupants more than 35 years ago. It claims that the news of the crashed saucer was kept secret from Army Chief of Staff Gen. Dwight D. Eisenhower because "he did not possess the necessary clearances to be permitted access to such information" and that it was withheld from him even after he became president.

Curiously, 38 of the 46 books listed in the main bibliography are strongly pro-UFO, and only two are skeptical. (One of these two has been out of print for a decade.) Students and teachers are informed that 37 of the recommended books can be purchased from Hynek's own Center for UFO Studies. It also plugs the Center's magazine and catalog of materials.

The main bibliography does not list two of the leading UFO critics' books: *UFOs Explained,* by Philip J. Klass, and *The UFO Verdict* by Robert Sheaffer. (Klass is chairman of CSICOP's UFO Subcommittee and Sheaffer is vice-chairman.) These were later listed in an afterthought addendum, not in the recommended-books section.

Crabbe herself was enthusiastic about UFOs being chosen as this year's topic—based on a ballot of former contestants, coaches, and teachers—because "it would really capture their

[students'] imagination and get them involved and enthusiastic . . . It's certainly not dull, as some [subjects] might be . . . It's provocative." She added: "Our role is not to try to persuade children *what* to think, but rather *to think;* after reading materials and talking to persons, they hopefully will reach their own conclusions about each of the topics presented."

The late Edward U. Condon, former director of the National Bureau of Standards, and head of the University of Colorado UFO investigation in the late 1960s, offered the following advice in his final report on the study: "A related problem to which we wish to direct public attention is the miseducation in our schools which arises from the fact that many children are being allowed, if not actively encouraged, to devote their science study time to the reading of UFO books and magazine articles . . . We feel that children are educationally harmed by absorbing unsound and erroneous material as if it were scientifically well founded. Such study is harmful not merely because of the erroneous nature of the material itself, but also because *such study retards the development of a critical faculty with regard to scientific evidence, which to some degree ought to be part of the education of every American."* (Emphasis added.)

—Philip J. Klass

NOVA's UFO Program: Who Was in Charge?

As Robert Sheaffer mentions in his "Psychic Vibrations" column in this issue, UFO proponents upset about the NOVA program "The Case of the UFOs" have complained in print that the Editor of *SI* selected the participants in the program. J. Allen Hynek, in a guest editorial in the MUFON *UFO Journal,* even says I selected the *final* participants in the telecast and that this therefore explains the program's alleged bias against pro-UFO spokesmen. While I am pleased to have my name associated with what was generally a well-done program, alas, I cannot claim any of the credit.

The facts are these:

The BBC producer John Groom made a very preliminary phone call to me early in 1982, prior to his beginning any travels or interviews.

He asked my advice on who might contribute to the program, a question I assume he asked of everyone he talked to.

I gave him the names of five persons.

Two of those five persons ended up being in the program.

The work of both of them was already familiar to Mr. Groom when he called me.

About a dozen other people appear in the program; I neither had mentioned them to Groom nor had anything to do with their "selection."

My only other communication with Groom was about a month after the first, when he called to see if we might be able to meet when he made his first trip to the United States. Our schedules did not permit it.

To this day I have not had any other communication with Mr. Groom.

The attempt of disgruntled UFO proponents to rationalize the program's critical, investigative approach to UFO reports as somehow being due to a "bias" of the SKEPTICAL INQUIRER has no factual support. I assume Mr. Groom based his editorial judgments on his own assessment of the value and credibility of individual UFO witnesses and investigators.

—K. F.

Anti-pseudoscience in Austria

A new magazine dedicated to combating superstition and pseudoscience has begun publication in Austria. Called *Contra*, its founder and editor is Walter Hain, a Vienna writer and editor.

Contra's first issue, October 1982, has a story on the hoax concerning the caves of Ecuador promulgated in Erich von Däniken's *Gold of the Gods*, including new information from Miami, Florida, archaeologist Pino Turolla (see *SI* Fall/Winter 1977, p. 32, for earlier report). There is also an examination by Klaus Webner of the Nagora UFO incident, a discussion with the French astrologer Elisabeth Teissier and the German critic Hoimar von Ditfurth, a story about the cart-ruts of Malta, and an article entitled, "What Is Superstition, What Is Pseudoscience?" Hain says he is looking for new stories (in German), reports, and collaborators. For further information write to

Zeitschrift gegen Aberglauben und Pseudowissenschaft
Contra
Nagora-UFO entlarvt
Erich von Däniken doch ein Schwindler
Malta — Neue Erklärungen der Karrenspuren
Elisabeth Teissier im Kreuzverhör
Was ist Aberglaube, was ist Pseudowissenschaft

Hain at *Contra*, Donaufelderstr. 36, A-1210 Wien (Vienna), Austria.

—*K.F.*

Insights and Ideas

Skeptical scrutiny is the means, in both science and religion, by which deep insights can be winnowed from deep nonsense. . . . The well-meaning contention that all ideas have equal merit seems to me little different from the disastrous contention that no ideas have any merit.

—Carl Sagan in *Broca's Brain*

Testimony of Miracles

No testimony is sufficient to establish a miracle, unless the testimony be of such a kind that its falsehood would be more miraculous than the fact which it endeavours to establish.

—David Hume, "On Miracles," in *An Enquiry Concerning Human Understanding* (1748)

Psychic Vibrations

You may have seen, and enjoyed, the NOVA program titled "The Case of the UFOs," shown this past October. It was generally quite careful with facts, and reasonably skeptical—as befits a science-oriented program—yet it did not close the door on the possibility that there may in fact be some unknown phenomenon responsible for some UFO reports. Nonetheless, officials of MUFON—the Mutual UFO Network, whose members generally believe in "UFO abductions" and crashed flying saucers in the New Mexico desert—are furious. MUFON director Walt Andrus accused the producers of the show of "leaving out the people who could contribute positive information"—in other words, those who believe in visits from little men from space. Andrus places much of the "blame" for the way the show turned out on *SI*'s own Kendrick Frazier, who gave the producers the names of people to contact. Two CSICOP fellows and several other skeptics appeared on the program, in addition to several proponents of the reality of UFOs. Andrus suggests that Frazier failed to warn the producers about "the biased backgrounds on these men." J. Allen Hynek, founder of the Center for UFO Studies, wrote an article published by both MUFON and his own UFO center's newsletter expressing, in Andrus's words, "his disgust and contempt with this obvious UFO debunking program."

* * * * *

Henry Gris, the *National Enquirer*'s chief news hound for stories about "paranormal" occurrences behind the Iron Curtain, has scored another exclusive scoop: "Russians Float in Midair—Lifted Only by Hypnosis." Gris claims to have witnessed floating cosmonauts in a secret research lab somewhere near Moscow. "I'm absolutely convinced it was real," says Gris. A Soviet scientist reportedly explained that "as they are put under hypnosis, their nervous system is ordered to become insensitive to the actual physical weight of their bodies." Gris recently revealed another startling development in Soviet science: "Russians Halt Aging Process." He is one of the principal sources of "reliable" information about UFO sightings and research in the USSR.

* * * * *

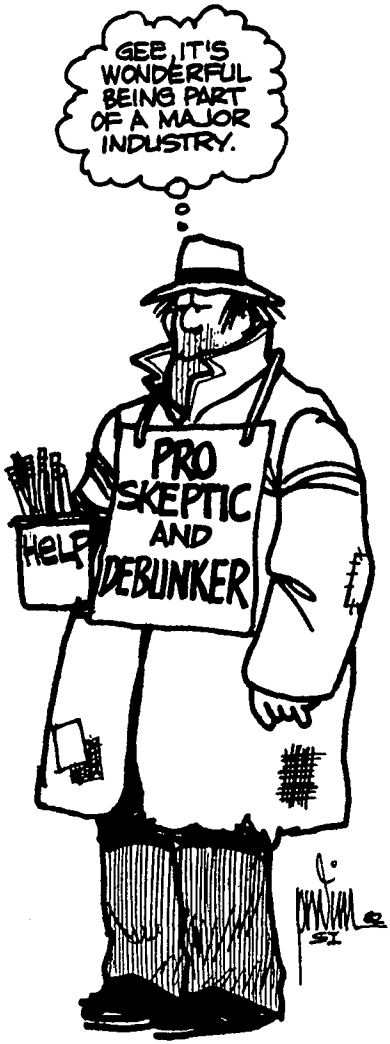
Yet another new publication joins the ranks of those seeking to cash in on interest in the occult: *The Reincarnation Report*. Among the exciting articles to appear in its pages: "Communicating With Your Unborn Child—Who

was your child in a previous life and what is he returning to accomplish in this lifetime? Several mothers have received startling information." Also: "The Sexual Influence of Past Lives"; "A Psychic Told Me This Is My Last Life"; and perhaps the most important, "The Rebirth Planning Session—Four hypnotic regression sessions in which subjects talk about their final preparation for rebirth."

* * * * *

Two UFO witnesses continue to make news years after their well-publicized sightings. Quentin Fogarty, key witness in the New Zealand UFO sighting and filming from an aircraft on New Year's Eve 1978, has now written a book about his experience titled *Let's Hope They're Friendly*. Interviewed in the Melbourne newspaper the *Age*, Fogarty reveals that "he is inclined to think that the lights were in some way spiritual, perhaps supernatural." Fogarty said that he recalls feeling at the time that the lights were collecting the souls of the dead. (In a 1979 article in an Australian magazine, Fogarty revealed that he "felt a presence in the back of the plane" but said nothing.) He attributes the failure of his story to win universal acceptance to the UFO debunking "industry." Of UFO skeptics, Fogarty says, "That's how they make their living. They write books on it, they lecture on it, just as the UFO believers do. But it's a big industry, and if you get in the debunkers' sights they'll try to blow you away." I am one of those supposed "professional debunkers" to whom Fogarty refers. In 1981 I received all of 1.2 percent of my total income from writing and lectures. In 1979, my best year yet, it was 3.4 percent. Other UFO skeptics report equally piddling sums. "It's a big industry" indeed. One hopes that Fogarty's account of the UFO sighting is not as wildly exaggerated as his statements about persecution by money-seeking UFO skeptics.

Meanwhile, Kenneth Arnold, the man whose June 1947 sighting from an aircraft started the whole UFO mania, is now saying that the UFOs he sighted may have been a kind of link between the world of the living and the world of spirits. Arnold, whose description of the objects he saw as looking like



"saucers skipping over the water" gave rise to the term "flying saucers," said in an interview in *UFO Review* that "there might be two worlds connecting the living and the dead." He says he has seen many more UFOs since that first sighting in 1947: "Once I saw a UFO which changed its density, so I concluded these things could be something alive rather than machines." He repeated the claim, published as long ago as 1969, that invisible entities once entered his home, beings he speculates came from the UFOs. "I was aware of their presence because I could see my rugs and furniture sink down under their weight." If Arnold's story had not been believed in 1947, it is quite conceivable that we would not have UFO sightings today.

* * * * *

The August/September 1982 issue of the Center for UFO Studies *Associate Newsletter* breaks new ground in scientific UFOlogy with "First Reported CE-III Alien Communicating with Deaf-Mute." William Ortiz, originally from Colombia but now living in the U.S., claims to have had three UFO encounters. On two of these occasions, contact with aliens temporarily restored his hearing. Apparently the aliens knew that he was unable to hear and so used hand signals to communicate with him. Ortiz has sketched the aliens he reportedly saw as well as their sign-language gestures. CUFOS does not firmly endorse Ortiz's claim, saying, "Many questions about the case remain unanswered," but they reported that "Dr. Hynek has met and communicated with the witness and believes the information received should be noted by other investigators of the UFO phenomenon." The case was first reported by CUFOS investigator Virgilio Sanchez-Ocejo. Apparently CUFOS is unaware that their man Sanchez-Ocejo is coauthor of a sensationalist book titled *UFO Contact from Undersea*. It claims that extraterrestrials have established bases under the earth's oceans and that UFO "abductees" have been taken there. The other author of this book is none other than Wendelle C. Stevens, the man who tirelessly championed the outrageous "UFOs from the Pleiades" hoax. (Even most UFO believers have rejected that claim.) Once again, "scientific" UFOlogy has shown a remarkable inability to be on guard against those who make wild claims.

* * * * *

In January 1982, the *Globe*, a tabloid, published a photograph taken from an airplane window purporting to show an angel descending from the sky. Within two months, the *Globe* reported that "hundreds of readers have now sent us

startlingly similar pictures." They have found that "the heavenly visitor seems to appear most often outside airplane windows, at funerals, and during violent storms."

* * * * *

Another tabloid, the *Weekly World News*, reports: "MYSTERY OF THE TALKING WALL: Experts record voices from 700 years ago." It seems that the bricks of an ancient tavern in Wales have allegedly stored the sound of thirteenth-century merrymaking. After the owner of the pub reported hearing voices and organ music, two engineers investigated the phenomenon and reportedly succeeded in making tapes of the eerie sounds. A journalist who heard the tapes said that he could make out voices and music, adding, "It certainly isn't a hoax. I heard the voices of men laughing and talking. It was clear enough, but not a language I could understand. It must have been in some ancient Welsh dialect." One of the researchers theorized that "the walls contain a mixture of silica and ferric salts just like those on recording tape. There is no reason why it shouldn't be able to record sound."

—*Robert Sheaffer*

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Iridology: Diagnosis or Delusion?

Does the iris really record the health of each organ? An examination of a medical pseudoscience of rising appeal.

Russell S. Worrall

For centuries the eye has been said to be the mirror of the body and the soul. Terror and love are expressed through the eyes; and the general state of health can be reflected in the eyes, as in the vacant, glassy stare of the gravely ill. Today, a more precise analogy would be to describe the eye as a window rather than a mirror. The eye is an optically clear porthole that allows one to view body tissues, such as blood vessels and nerves, in their undisturbed state. The subject of this paper, iridology, proposes a more elaborate analogy—specifically, that the iris of the eye is a gauge registering the condition of the body's various organs; or, as Jessica Maxwell describes it in her book *The Eye/Body Connection*, the iris is "an organic Etch-a-Sketch" (Maxwell 1980, p. 12).

Iridology, pronounced "eyeridology," is the "science" of reading the markings or signs in the iris (the colored part of the eye) to determine the functional state of the various components of the body. It is not unique; other, equally sophisticated systems of belief exist utilizing the soles of the feet, the ear, the palm [see *SI*, Winter 1982-83], and the spine. A common theme unites these varied techniques. They are noninvasive (you do not have to be punctured or sliced into!) and they each involve a specific area of the body's surface, which when read by a "well-trained" practitioner reveals your innermost health problems. Elaborate charts (suitable for framing) that guide the practitioner and impress the patient are the centerpiece of each method.

Iridology may have its origins in antiquity, or more recently in Russia, as suggested by the *National Enquirer* (1978), but Dr. Ignatz von Peczely, of Hungary, is generally held responsible for developing and promoting

Russell S. Worrall is an assistant clinical professor in the School of Optometry, University of California, Berkeley.

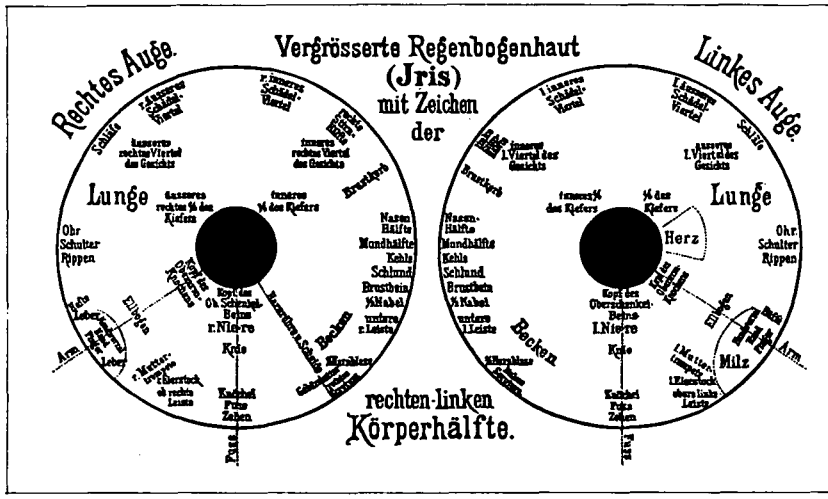

FIGURE 1. An iris chart developed by von Peczely.

the modern "science" of iris diagnosis. According to the story, the miraculous discovery of the iris-body connection came when, as a boy of ten, Ignatz accidently broke the leg of an owl and a black stripe spontaneously appeared on the owl's iris. He went on to develop charts based on his clinical observations (see Figure 1) and published a book on the subject (Von Peczely 1866).

In the United States, Bernard Jensen has been the most influential proponent of iridology. His book, *The Science and Practice of Iridology* (Jensen [1952] 1974), has been the authority on the subject since its publication in 1952. His new book, *Iridology: The Science and Practice in*

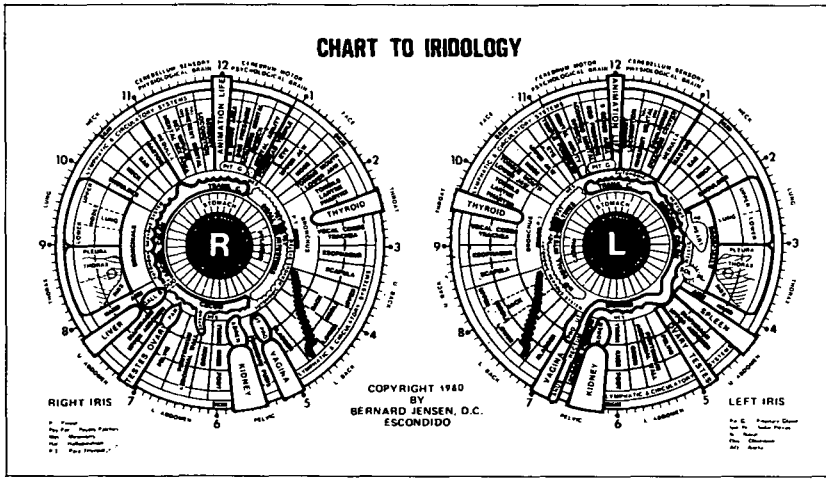

FIGURE 2. Iridology chart by Jensen (Jensen, *Iridology Simplified*, Iridologists International, 1980).

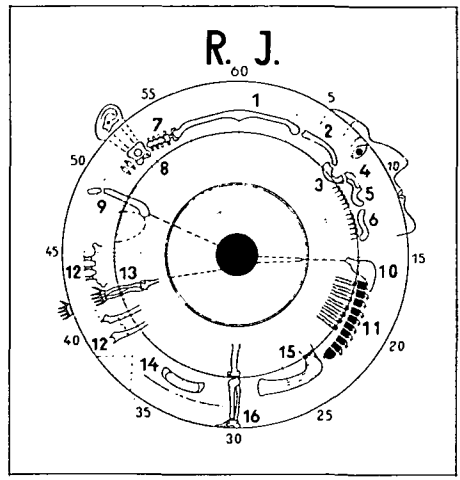

FIGURE 3. Schematic representation of the Bone Zone of the right eye by Kriege: (1) Cranial bone. (2) Frontal bone. (3) Orbit. (4) Nasal bone. (5) Upper jaw and teeth. (6) Lower jaw and teeth. (7) Cervical vertebrae. (8) Ear. (9) Shoulder and clavicle. (10) Scapula. (11) Spine and ribs. (12) Sternum and ribs. (13) Hand and arm bones. (14) True pelvis. (15) Pelvic crests. (16) Foot and leg bones. (*Fundamental Basis of Irisdiagnosis*, trans. Priest, Fowler Co., London, 1975, p. 102).

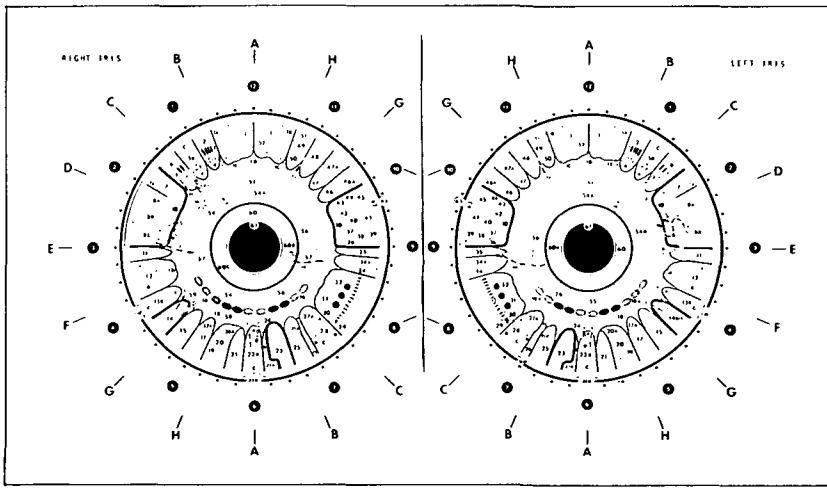

FIGURE 4. Basic European Iridology chart by Korvin-Swiecki. Some examples from the chart: (1) Cerebrum. (2) Cerebellum. (5) Ear. (6) Neck, throat. (8) Lungs. (9) Heart. (B) Aorta. (14) Liver. (16) Pancreas. (23) Kidneys. (27A) Uterus. (42) Larynx. (53) Autonomic nervous system. (54) Ascending colon. (58) Appendix. (59) Gallbladder. (60) Stomach. (61) Central nervous system. (62) Circulatory and lymphatic system. (Jessica Maxwell, *The Eye/Body Connection*, Warner Books, New York, 1980, pp. 60-63)

the Healing Arts, vol. 2 (Jensen 1982), is a 580-page epic detailing the history, "science," and application of iridology. Dr. Jensen's updated charts (see Figure 2) are the standard in the United States. European charts, though more detailed and exacting, follow the same general format

as illustrated by Kriege's (1975) Bone Zone Chart (Figure 3) and Korvin-Swiecki's charts (Figure 4) shown in *The Eye-Body Connection* (Maxwell 1980). This pseudoscience has been popularized by many recent books and articles, including a story in the *National Enquirer* (1978) with a typically dramatic headline: "Do-It-Yourself Eye Test That Can Save Your Life." All these reports are blandly neutral or, more commonly, outright enthusiastic about this purportedly marvelous diagnostic procedure. Only two papers present a critical view based on controlled scientific studies, the results of which are not astonishing to the skeptical inquirer.

The philosophical, scientific, and clinical ramifications of iridology are succinctly stated by Harri Wolf (1979, pp. 7-8), founder of the National Iridology Research Foundation, in the introductory remarks of his *Applied Iridology:*

> Now wouldn't it seem *logical* [emphasis added] that through some creative design, or evolutionary process (whatever the reader's preference), the human body would be equipped with a metering device functioning as a gauge in regard to the health of the individual?
>
> Each of us is, *in fact* [emphasis added], equipped with just such a miniature recording screen—the iris. Via the direct neural connection of the surface layers of the iris with the cervical ganglion of the sympathetic nervous system, impressions from all over the body are conveyed to the iris. Thus is established the neuro-optic reflex.
>
> Iridology, as the study of the neuro-optic reflex is known, is the art/science of *revealing* [emphasis added] pathological, structural and functional disturbances in the body.

The Philosophy and Logic

The claim that iridology is a logical, natural system is central to iridology philosophy. As Jensen (1981a, p. 2) writes, "We must realize that iridology represents a law of nature that cannot be changed. I believe that it is just as immutable and unchangeable as any of the laws that govern the universe." When viewed from a critical perspective, the logic in iridology begins to fade. First, a gauge or metering system has to be read and understood to be useful. The iris of the eye is certainly inaccessible to all of earth's creatures, including man (unless he happens to have a mirror handy). Further, the iris signs are so complex as to be unintelligible to all but those who have been enlightened by von Peczely's theories. In short, there is no logical evidence to support a claim of functional utility for this complex biological system purported to exist in many diverse organisms, including man.

This apparent lack of utility to the organism exposes a more fundamental flaw in the logic of iridology when it is considered in the context of the evolutionary process. A physiological subsystem such as the suggested iris-body connection would be developed and refined under the gradual pressures exerted by natural selection. For such a system (more

properly the gene pool that codes for the system) to have evolved in many diverse species, a distinct survival advantage had to be present for the organisms with this system. This is an assumption for which I can offer no logical arguments. Certainly the saber-toothed tiger derived little benefit from this amazing metering system, nor does today's modern owl.

The Science

A second theme in iridology is that, as Wolf states, the neuro-optic reflex exists "in fact." The facts supporting the existence of the neuro-optic reflex are tenuous at best. It is postulated by Wolf (1979, p. 7) that the sympathetic division of the automatic nervous system mediates the iris response. D. Bamer (1982, p. 22) includes the parasympathetic division in his theory and offers an anatomical diagram to support his claim (see Figure 5). In a gross anatomical sense the autonomic system does interconnect and enervate almost every segment of the body, including the eye. However, anatomical interconnection does not imply functional connection any more than having a telephone in your home is proof of the proposition that you receive all of the calls intended for the president of the United States. Further, the autonomic nerves supplying the eye are of small caliber and would not seem to have adequate numbers of nerve fibers to handle the volume of information presumed to reach the iris. Anatomical, physiological, and clinical studies have eloquently

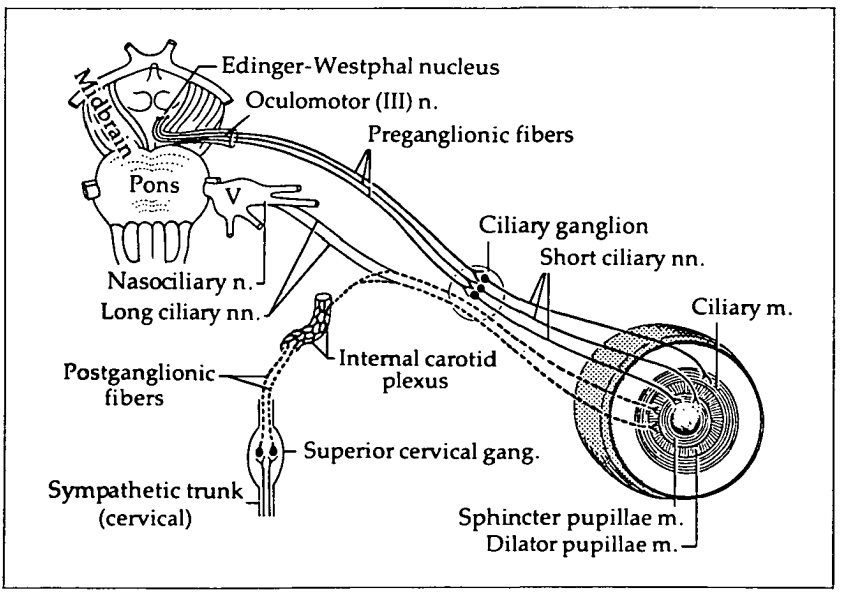

FIGURE 5. Anatomical diagram by Bamer (*Applied Iridology and Herbology*, BiWord Publishers, Orem, Utah, 1982, p. 22)

demonstrated the functional neural pathways involved in many of the eye's control and response mechanisms, but published studies report no evidence in support of a functional iris-body connection (Moses 1975; Last 1973). Though these investigations were not specifically looking for a neuro-optic reflex, given the quality and quantity of information postulated to appear in the iris it is curious that even accidental detection of this elaborate system has eluded researchers.

One aspect of the functional theory, as expressed by Jensen, is especially interesting in light of well-established neurological evidence. Anatomists and physiologists have long known that as a general rule the central nervous system is functionally split, with each side controlling and monitoring the opposite side of the body. In iridology it has also been "established" that each eye "sees" its own side of the body. Thus a conflict is created for iridologists in explaining the flow of information over the autonomic pathways that cross to the opposite side as they travel through the central nervous system. To explain this apparent difficulty, Jensen proposes that the optic nerve serves as the final link between the autonomic system and the iris (see Figures 6 and 7). Since the optic nerve crosses between the eye and the brain, information from an organ would make a second crossing on its way to the iris and register on the same side that it originated from. Jensen (1980, p. 3) also infers that the large size of the optic nerve would provide the needed transmission capacity to account for the flow of information to the iris. Thus, with Jensen's assumptions, iridology theory seems to agree with anatomical evidence!

The assumption that the optic nerve mediates the final leg of the

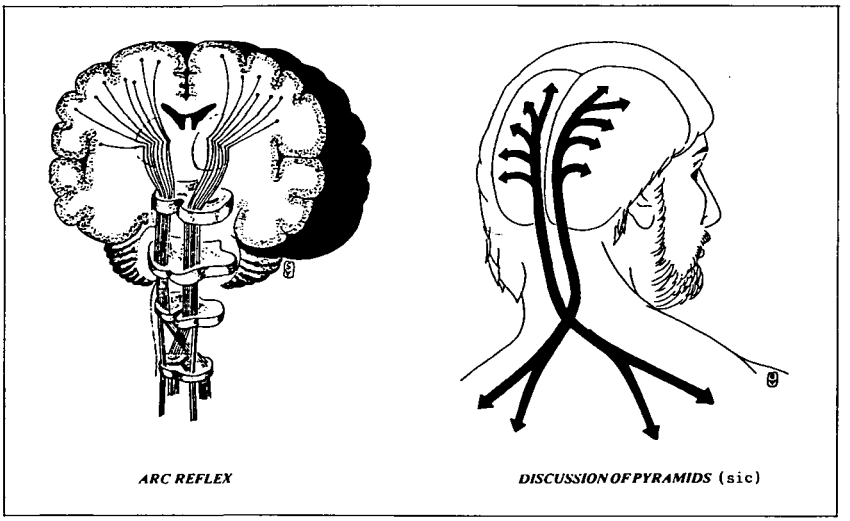

FIGURE 6. The first crossing in the autonomic system according to Jensen. (*Iridologists International Manual for Research and Development*, Iridologists International, Escondido, Calif., 1981, p. 25)

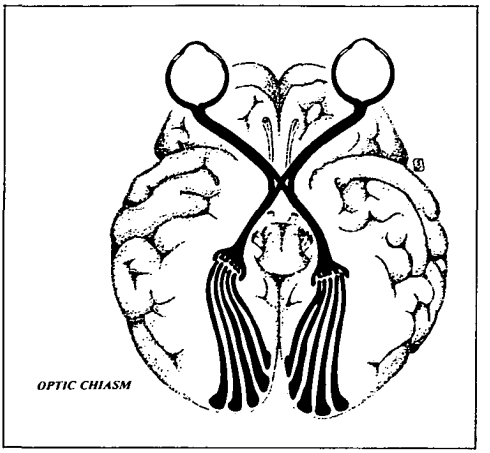

FIGURE 7. The second crossing in the optic nerve according to Jensen (*Iridologists International Manual for Research and Development*, Iridologists International, Escondido, Calif., 1981, p. 27)

neuro-optic response solves the iridologists' theoretical dilemma by creating a double cross, but at the same time the assumption raises serious questions by anyone familiar with the tremendous body of literature published on the visual pathways. The visual system (including the optic nerve) is probably the most intensively studied and best understood neural system in the body. The Nobel Prize recently awarded to Hubel and Wiesel was the result of many years of work on this intriguing system. All of the accumulated research unequivocally demonstrates that the mammalian optic nerve is primarily an afferent pathway, that is, one in which the signals travel from the eye to the brain. There is no evidence suggesting that any fibers from the optic nerve make connections with the iris. This, combined with the fact that only half of the fibers in the optic nerve cross, makes the proposition that the optic nerve is the final link to the iris untenable (Moses 1975, pp. 367-405).

This double-cross hypothesis is characteristic of the "scientific" evidence presented in Jensen's new text. This volume contains countless misinterpretations of established anatomical and physiological knowledge and includes references to many pseudosciences, such as Kirlian photography and personology (Jensen 1982, pp. 88, 491).

This pseudoscience rises to the level of the ridiculous with the proposition that the iris can communicate information back to remote organs. D. Hall (1981, pp. 210-11) describes the "removal of function" following the surgical removal of a piece of iris tissue in glaucoma or cataract surgery. She says, "As sure as eggs, the iris zones affected will be down in function and maybe in structure too." As D. Stark (1981, p. 677) points out, "This can only be the case if the iris not only reflects but also controls bodily function, which is patently absurd."

Clinical Considerations

Though the gossamer theoretical structure that underlies iridology is apparent to the critical inquirer, proponents and practitioners continue to sell iridology as a "valid" clinical procedure. As Wolf (1979, pp. 7-8) states, iridology as a clinical tool reveals "pathological, structural and functional disturbances in the body." Jensen (1981a, p. 2) adds that "iridology is unique in its ability to make a subclinical evaluation, whereas the medical point of view only recognized lab test verification of dysfunction." Although Jensen mentions "diagnosis" in many of his writings, he qualifies this by stating (1981b, p. 16) that "iridology does not diagnose disease in the sense that Western medicine does, nor does it label combinations of symptoms with disease names." A paper by Fernandiz appearing in the same journal (published by Jensen) is titled "Hemicrania (Migraine) and Its Diagnosis by Means of the Iris"! Such double talk does not cover the reality of the fact that iridology is a clinical technique purported to determine functional states within the body for the purpose of recommending a course of corrective treatment.

Confusion is the first order of business in the clinical application of iridology; for, as Stark (1981, p. 677) notes, there are many iris charts (more than 19) and this "presents the first diagnostic dilemma—which chart to choose." Although most charts are in general agreement on major landmarks, such as the leg area being represented at the six-o'clock position, there are also many differences in both location and interpretation of iris signs (compare Figures 2-5).

Acknowledging the lack of objective clinical evidence, in a recent article Jensen (1981a, p. 1) says, "At the present time, we have no exact way of proving anything other than in a phenomenological manner that what we know and see is true." Thus, in support of the efficacy of iridology, proponents have published endless numbers of anecdotal case reports.

Medicine has long used clinical observation to support claims of observed phenomena even when detailed knowledge of the underlying functional processes are not fully understood. To ensure reliable reproducible results, clinical investigators have adopted strict rules in the form of controlled clinical studies. The controls are chosen to remove, to the extent possible, the inevitable bias of both the patient and the practitioner, to isolate the procedure or medication, and to provide subject groups of adequate size to make statistical comparisons. The controlled clinical study is not, as promoters of iridology suggest, a tool developed by Western medicine to attack unorthodox procedures (Jensen 1981b, p. 20), but in fact it is universally accepted and applied in all areas of scientific inquiry.

The design of a controlled clinical study of iridology would at first appear relatively simple, especially in light of Jensen's (1974, p. 2) comment that "iridology can diagnose a patient for the doctor if he has a perfect colored photograph showing three-dimensional depth of the

patient's eye. The patient need not be present." Utilizing photographs in a study effectively isolates the iridologist from the patient, thus limiting the data available to only that obtained from the iris appearance. This eliminates the possibility that direct observation might provide information from general physical appearance and also prevents items of pertinent history from being inadvertently divulged, as is commonly observed in "cold readings" by psychics. The difficulty develops when a standard criterion for the diagnosis of the condition in question is established, as illustrated in the following discussion.

Establishing a standard diagnosis with which to compare the validity of iridology may well be impossible. First, as was quoted above, iridology claims to be able to make a subclinical diagnosis, that is, before symptoms or measurable signs develop. In addition, Jensen (1974, p. 12) adds: "Many times the conditions revealed in the iris today will not be apparent in the body for years to come, but time will inevitably show the analysis to be correct." Thus the proponents of iridology have an excellent but inherently unprovable explanation for the high rate of overdiagnosis (false positives) in controlled studies.

There is also little common diagnostic ground, because many of the conditions detected by practitioners of iridology are "diseases" whose existence has been disputed or discredited by scientific investigation. A common finding is a toxic bowel settlement (which is treated with procedures of questionable value, such as colonic irrigation!); however, the toxic settlement theory of disease was soundly discredited in the early part of this century (Ratclift 1962, p. 52). Thus from a critical perspective it would be difficult to agree on a standard diagnosis where the existence of the disease itself is in dispute.

Though the clinical application of iridology is widespread, the results of only two controlled clinical studies have been published. At the University of Melbourne (Australia), D. Cockburn compared iridology evaluations with known medical histories. The most interesting phase of his study had iridologists evaluate before-and-after iris photographs of subjects who developed an acute disease. He asked the iridologists to determine if a change in the iris had occurred and, if possible, to tell which organ was affected. The only set of photographs determined to have changes was a set taken as a control on the same subject two minutes apart! Cockburn (1981, p. 157) states, "It must be concluded that, at least for the subjects of the prospective trial and for the acute stage of the disease states represented, there were no detectable iris changes of the type depicted in the commonly used iris diagnosis charts."

At the University of California, San Diego, A. Simon, D. Worthen, and J. Mitas (1979) compared the accuracy of iridology based on the reading of color slides for the detection of kidney dysfunction. A blood chemistry test (creatinine level) was used as the standard for assessment of kidney function. Photographs of 143 subjects (48 with kidney disease) were read by three iridologists and three ophthalmologists. The overall record

for hitting a correct determination was no better than chance when the number of incorrect and correct determinations were compared. In conclusion the authors state, "Clearly, none of the six observers in this study derived data of clinical importance or significance" (p. 1389).

Jensen, one of the iridologists participating in the San Diego study, has written several critical commentaries on the results. His first criticism was the poor quality of the photographs; however, at the time of the study he did not decline to read them. He also disputes the validity of the creatinine test as an indication of kidney function, though it is widely accepted and routinely used by orthodox practitioners. He asserts that the creatinine test has been around for 10 to 12 years, whereas iridology has been in use for more than 125 years (Jensen 1981c, p. vi). Jensen seems oblivious to the fact that the amount of time a test has been in existence has no relevance to its validity.

As further evidence of the clinical value of iridology, Jensen cites the work of Romashov and Velkelvor of the USSR (*National Enquirer* 1978), who reported a 95-percent accuracy in 1,273 subjects with diagnosed disease. He also states that Deck in Germany has reported a 92-percent efficiency in the detection of kidney disease through iris diagnosis (Jensen 1981c, p. 19). Jensen does not describe the details of these investigations, the nature of the controls, or the standards used for diagnosis. These are important, because one iridologist in the San Diego study also could boast of having correctly identified 88 percent of those with kidney disease. Unfortunately, he reported that 88 percent of the normal subjects included in the study as a control were also suffering from kidney disease (Simon, Worthen, and Mitas 1979, pp. 1387-88). Therefore, without specific details of the design, the use of these studies is of no value when offered in support of iridology.

To enhance the image of iridology by association with an accepted clinical technique, Jensen (1981c, p. i) writes: "The fundus examination, which has been accepted by ophthalmologists, reads the arterial circulation. Similarly, iridology reads the iris stroma . . ." The word "similarly" is loosely applied in this analogy. The fundus examination is a routine medical procedure using a special optical instrument (ophthalmoscope) that provides a view of the interior lining of the eye through the pupil. Body tissues, including blood vessels, can be studied undisturbed, an opportunity not afforded elsewhere on the body. The changes in arterial appearance in the eye represent a local manifestation of a more generalized vascular disease. No specific reflexive communication with remote body organs needs to be postulated to explain this or any of the many other observed phenomena in a traditional fundus examination.

Science or Rhetoric

In his rhetorical war with "Western" medicine Jensen (1980, p. 2) writes: "Iridology is based on scientific observation. It is the kind of science that

cannot be related through scientific tests, for it does not provide clinical information." If this non sequitur does not deter all of those inclined to subject iridology to controlled clinical studies, Jensen (1981c, p. vi) adds the ultimate argument used by practitioners of all unorthodox procedures. He writes: "Iridology can only be judged by those who use it properly. Iridology has not been properly used by those who have criticized and say it fails the test." In other words, you have to be "sensitive" to the technique to ensure favorable results!

Even though proponents may have used iridology "properly" since Von Peczely published his theories in 1866, they have failed to publish even one well-documented study to support the validity of any of the information presented on their iris charts. Since efficacy has not been established, the ultimate question faced by practitioners of iridology is one of ethics in their relationship with patients.

Harmless Fad or Health Hazard?

It is clear from a logical, theoretical, and clinical perspective that iridology is a pseudoscience of no clinical value. Unfortunately, the use of iridology by unorthodox practitioners is all too common today, and the unsuspecting and often vulnerable patient in the clinical application of the "science" is the recipient of its presumed benefits. In my private practice and as a member of the faculty of the School of Optometry at the University of California, Berkeley, I have been increasingly alarmed by the growing popularity and acceptance of iridology as a diagnostic tool. This is an area of great concern to everyone in the health professions, because acceptance of this pseudoscience can lead an individual to delay needed treatment when a false-negative diagnosis is made (i.e., when a disease is present but not detected). This would appear inevitable given Jensen's recent advice on the differential diagnosis of appendicitis. He writes: "When trying to distinguish between appendicitis and cecal inflammation, we must carefully examine the area at five o'clock in the right iris. Many cases of cecal inflammation have been incorrectly diagnosed as appendicitis, but the iris reveals the location of the inflammation" (Jensen 1982, p. 235). (Remember Jensen claims not to diagnose!) A delay created while treating "cecal inflammation" could prove fatal if appendicitis is the correct diagnosis. On the other hand, when a false-positive finding is reported (i.e., when a disease is "detected" but not present) to a naive patient, extreme mental anguish can result. In addition, the patient may expend large sums of money on unneeded treatments or (if they are skeptical) on traditional diagnostic tests to confirm the reported nondisease.

The Formula for Success?

It would seem that the false-positive diagnosis of subclinical disease is the

underlying key to the popularity and success of iridology. The bulk of diseases reported are vaguely stated conditions in organs, such as an "underactive" pancreas or "chronic weakness" in the lungs. Such vagueness permits clinicians to capitalize on any improvement in the way a patient "feels" as proof that the treatments are doing some good. Under those conditions the cure rate and patient satisfaction in a clinical practice can be very high.

Though the validity of the diagnosis and treatment may rest on false premises, many patients appear to experience a positive change in their health. This is understandable since these programs often include a good diet and moderate exercise, a formula that would do us all some good! However, as the following two cases will illustrate, the false-positive diagnosis can also have a negative impact on the patient.

A well-educated accountant, whom I have seen routinely for eye care, was experiencing lower back pain. He consulted a local chiropractor, and during the course of treatment an iridology workup was recommended. The results indicated, among many other health problems, the presence of cancer. Overwhelmed, the patient spent the day in torment. Unable to consult his family physician, who was out of the office, or his wife, who was at work, he finally sought my advice late in the afternoon. After a lengthy discussion I was able to allay his fears and he began to understand, in a more critical way, the complexities of a medical diagnosis. He wondered how an intelligent person like himself could be caught up in such a deep emotional web over such a diagnosis. This story fortunately had a pleasant ending. However, the outcome could have been much more serious since this patient is also suffering from a heart condition, which was not noted on the iridology evaluation!

Another patient in my office related her recent experience with an herbalist and iridology. Based on her iris photographs, she was given a list of herbs and advice supposedly needed to correct a long list of low-grade chronic conditions. The prescription for this long list was a total of over $200 worth of herbs. Considering herself to be healthy, she was skeptical and decided to save her money. This case also had a happy ending, but it leads me to wonder how many naive patients are investing in questionable treatments based on the results of questionable diagnostics?

A more humorous episode occurred recently when an investigative reporter had an iridology workup. She was told that "a whitish color emanating from the iris shows a lot of acidity and mucus throughout the body and could be from eating a lot of meat, bread and milk products. When told the reporter is a vegetarian, [the iridologist] said the acidity could be a reverse effect from eating too much fruit and vegetables." A classic example of clinical nonsense (Meyer 1982, p. 81).

Delusion or Diagnosis?

It seems that the pseudoscience of iridology has deluded both patient and

practitioner alike. The surge in popularity that iridology and its fellow pseudodiagnostic sciences are enjoying is not surprising. Iridology is "amazing," relatively simple to learn, and painless and, most important, it has that mystical attraction on which unorthodox theories and practices have thrived over the centuries.

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The Nazca Drawings Revisited: Creation of a Full-Sized Duplicate

Re-creation of a 440-foot Nazca figure on a Kentucky field shows how the Peruvian drawings were most likely made.

Joe Nickell

Called "Riddles in the Sand" (*Discover* 1982) they are the famous Nazca lines and giant ground drawings etched across 30 miles of gravel-covered desert near Peru's southern coast.

The huge sketch-pad came to public prominence in Erich von Däniken's *Chariots of the Gods?*—a book that consistently underestimates the abilities of ancient "primitive" peoples and assigns many of their works to visiting extraterrestrials. Von Däniken (1970) argues that the Nazca lines and figures could have been "built according to instructions from an aircraft." He adds: "Classical archaeology does not admit that the pre-Inca peoples could have had a perfect surveying technique. And the theory that aircraft could have existed in antiquity is sheer humbug to them."

Von Däniken does not consider it humbug, and he obviously envisions flying saucers hovering above and beaming down instructions for the markings to awed primitives in their native tongue. He views the large drawings as "signals" (von Däniken 1970) and the longer and wider of the lines as "landing strips" (von Däniken 1972). But would extraterrestrials create signals for themselves in the shape of spiders and monkeys? And would such "signals" be less than 80 feet long (like some of the smaller Nazca figures)?

As to the "landing strip" notion, Maria Reiche, the German-born mathematician who for years has mapped and attempted to preserve the markings, has a ready rejoinder. Noting that the imagined runways are clear of stones and that the underlying ground is quite soft, she says, "I'm

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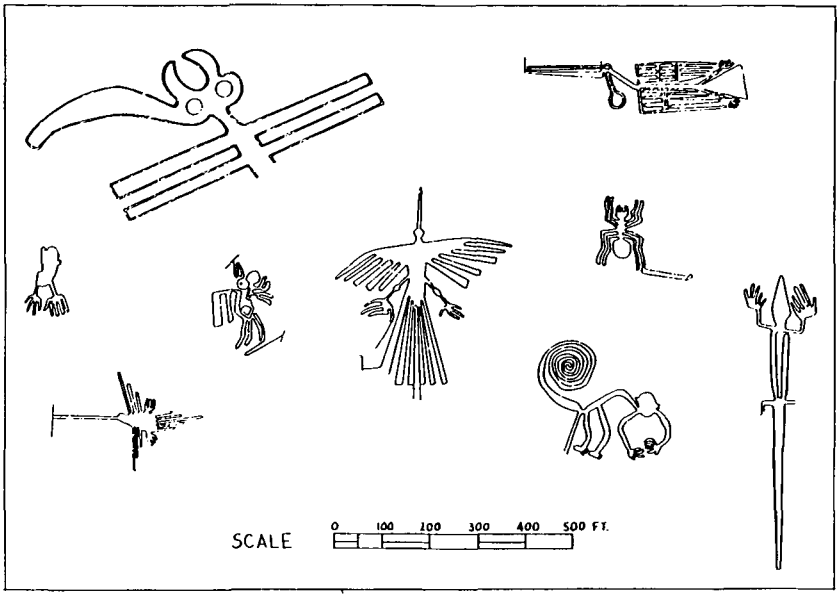

FIGURE 1. Etched upon the Nazca plains in Peru are giant drawings like these. Their large size has fueled misguided speculation that they were drawn with the aid of "ancient astronauts" or by sophisticated surveying techniques, the secrets of which are lost.

afraid the spacemen would have gotten stuck" (McIntyre 1975).

It is difficult to take von Däniken seriously, especially since his "theory" is not his own and it originated in jest. Wrote Paul Kosok (1947), the first to study the markings: "When first viewed from the air, [the lines] were nicknamed prehistoric landing fields and jokingly compared with the so-called canals on Mars." Moreover, one cropped photo exhibited by von Däniken (1970), showing an odd configuration "very reminiscent of the aircraft parking areas in a modern airport," is actually of the knee joint of one of the bird figures (Woodman 1977). (See Figure 1.) The spacecraft that parked there would be tiny indeed.

Closer to earth, but still merely a flight of fancy, in my opinion, is the notion of Jim Woodman (1977) and some of his colleagues from the International Explorers Society that the ancient Nazcas constructed hot-air balloons for "ceremonial flights," from which they could "appreciate the great ground drawings on the *pampas*." If one believes that the theory is also inflated with hot air, one must at least give Woodman credit for the strength of his convictions. Using cloth, rope, and reeds, Woodman and his associates actually made a balloon and gondola similar to those the Nazcas might have made had they actually done so. Woodman and British balloonist Julian Nott then risked their lives in a 300-foot-high fly-over of the Nazca plain. Their balloon was descending rapidly and after they had thrown off more and more sacks of ballast they jumped clear of their craft some ten feet above the *pampas*. Free of the balloonists' weight, the

balloon shot skyward and soared almost out of sight, only to finally crash and drag briefly across the ground.

The Nazca markings are indeed a mystery, although we do know who produced them—von Däniken notwithstanding. Conceding that Nazca pottery is found in association with the lines, von Däniken (1970) writes: "But it is surely oversimplifying things to attribute the geometrically arranged lines to the Nazca culture for that reason alone."

No knowledgeable person does. The striking similarity of the stylized figures to those of known Nazca art has been clearly demonstrated (Isbell 1978; 1980). In addition to this iconographic evidence must be added that from carbon-14 analysis: Wooden stakes mark the termination of some of the long lines and one of these was dated to A.D. 525 (±80). This is consistent with the presence of the Nazca Indians who flourished in the area from 200 B.C. to about A.D. 600. Their graves and the ruins of their settlements lie near the drawings.

The questions of who and when aside, the mystery of *why* the markings were made remains, although several hypotheses have been proffered. One is that they represent some form of offerings to the Indian gods (McIntyre 1975). Another is that they form a giant astronomical calendar or "star chart." Writing in *Scientific American,* William H. Isbell (1978) states:

> As Reiche has pointed out for many years, certain of the Pampa Colorada lines mark the position of the sun at the summer and winter solstices and certain other lines also appear to have calendrical significance. A computerized analysis of line orientation conducted by Hawkins, although it failed to demonstrate that a majority of the lines have astronomical significance, showed that twice as many of them were oriented with respect to annual solar and lunar extremes than would be expected on the basis of chance.

Isbell himself suggests that an important function of the markings was economic and "related to the drafting of community labor for public works," although at best that is only a partial explanation.

Still another suggestion (first mentioned by Kosok) comes from art historian Alan Sawyer (McIntyre 1975): "Most figures are composed of a single line that never crosses itself, perhaps the path of a ritual maze. If so, when the Nazcas walked the line, they could have felt they were absorbing the essence of whatever the drawing symbolized." Sawyer is correct in observing that most of the figures are drawn with a continuous, uninterrupted line. But there *are* exceptions, and it is possible that the continuous-line technique is related to the method of producing the figures, as we shall discuss presently.

In any case, these are only some of the hypotheses; whatever meaning(s) we ascribe to the Nazca lines and drawings must be considered in light of other giant ground-markings elsewhere. Even putting aside the

Japanese and European ones—e.g., the White Horse of Uffington, England, which is known from as early as the twelfth century (Welfare and Fairley 1980)—we are left with numerous ground drawings in both North and South America.

In South America giant effigies are found in other locales in Peru, for example, and in Chile, in the Atacama Desert (Welfare and Fairley 1980). Interestingly, the plan of the Incan city of Cuzco was laid out in the shape of a puma, and its inhabitants were known as "members of the body of the puma" (Isbell 1978; 1980).

Turning to North America, there is the Great Serpent Mound in Ohio and giant effigies in the American Southwest. In 1978, with the aid of an Indian guide, I was able to view the ground drawings near Blythe, California, in the Mojave Desert. Like the Nazca figures, the Blythe effigies are large and give the impression they were meant to be viewed from the air. Also in common with the Nazca figures, they were formed by clearing away the surface gravel to expose the lighter-colored soil. However, although they are thought to date from a much later period (Setzler 1952), none of the Blythe figures match the size of the largest Nazca drawings; and the human figures and horselike creatures are much cruder in form, typically having solid-area bodies and sticklike appendages—quite unlike the continuous-line drawings of Nazca (yet somewhat similar to some of the Chilean effigies). Moreover, absent from the Blythe site are the "ruler-straight" lines that may or may not have calendrical significance.

In short, there are similarities and dissimilarities between the Nazca and other ground drawings that complicate our attempts to explain them. Certainly the Blythe and other effigies have no attendant von Dänikenesque "runways"; neither do their crude forms suggest they were drawn with the aid of hovering spacecraft. And there is nothing whatever to warrant the assumption that they were made to be viewed by select native balloonists on aerial sorties.

It seemed to me that a study of *how* the lines were planned and executed might shed some light on the ancient riddle. English explorer and film-maker Tony Morrison has demonstrated that, by using a series of ranging poles, straight lines could be constructed over many miles (Welfare and Fairley 1980). (The long lines "veer from a straight line by only a few yards every mile," reports *Time* [1974].) In fact, along some lines, the remains of posts have been found at roughly one-mile intervals (McIntyre 1975).

By far the most work on the problem of Nazca engineering methods has been done by Maria Reiche (1976). She explains that Nazca artists prepared preliminary drawings on small six-foot-square plots. These plots are still visible near many of the larger figures. The preliminary drawing was then broken down into its component parts for enlargement. Straight lines, she observed, could be made by stretching a rope between two stakes.

Circles could easily be scribed by means of a rope anchored to a rock or stake, and more complex curves could be drawn by linking appropriate arcs. As proof, she reports that there are indeed stones or holes at points that are centers for arcs.

But Reiche does not detail the specific means for positioning the stakes that apparently served as the centers for arcs or the end points of straight lines. In her book she wrote, "Ancient Peruvians must have had instruments and equipment which we ignore and which together with ancient knowledge were buried and hidden from the eyes of the conquerors as the one treasure which was not to be surrendered." Be that as it may, Isbell (1978) states: "Maria Reiche, using scale models, has made major advances toward demonstrating how Nazca ground art was produced. Although more research needs to be done, the prehistoric engineering skills are no longer completely unknown."

Isbell himself suggests that the Nazcas used a grid system adapted from their weaving experience, a loom "establishing a natural grid within which a figure is placed." All that would be necessary, he observes, would be to simply enlarge the grid to produce the large drawings.

However, as one who has used the grid system countless times (in reproducing large trademarks and pictorials on billboards—summer work during my high school and college years), I am convinced the grid system was not employed. To mention only one reason, a characteristic of the grid method is that errors or distortions are largely confined to individual squares. Thus, the "condor" drawing in Figure 1—with its askew wings, mismatched feet, and other asymmetrical features—seems not to have been reproduced by means of a grid.

Other, even less likely possibilities would be the plotting of points by a traverse surveying technique (such as is used today to plot a boundary of land) or by triangulation. Having some experience with both of these, I note that such methods depend on the accurate measurement of angles, and there appears to be no evidence that the Nazcas had such a capability.

I decided to attempt to reproduce one of the larger Nazca figures—the 440-foot-long condor in the center of Figure 1—using a means I thought the Nazcas might actually have employed. I was joined in the project by two of my cousins, John May and Sid Haney. The method we chose was quite simple: We would establish a center line and locate points on the drawing by plotting their coordinates. That is, on the small drawing we would measure along the center line from one end (the bird's beak) to a point on the line directly opposite the point to be plotted (say a wing tip). Then we would measure the distance from the center line to the desired point. A given number of units on the small drawing would require the same number of units—larger units—on the large drawing.

For this larger unit we used one gleaned by Maria Reiche from her study of the Nazca drawings and approximately equivalent to 12.68 inches. For measuring on the ground, we prepared ropes marked off with paint

into these Nazca "feet," with a knot tied at each ten-"foot" interval for a total length of 100 units. To aid in accuracy in plotting on the ground, we decided to employ a "T" made of two slender strips of wood. With this we could ensure that each measurement made from the center line would be at approximate right-angles to the line.

My father, J. Wendell Nickell, took charge of logistics—including obtaining permission to use a suitable giant "drawing board" (a landfill area in West Liberty, Kentucky, owned by Dr. C. C. Smith, to whom we are grateful) and securing the services of a pilot for the subsequent aerial photography. Since we could not mark the lines by clearing gravel to expose lighter-colored earth, as the Nazcas did, we planned to simply mark them with white lime, as one marks a playing field. With the addition of my young cousin, Jim Mathis, and my 11-year-old nephew, Conrad Nickell, our work crew of Indians was complete.

On the morning of August 7, 1982, the six of us assembled at the site and immediately began by laying out the center line. Some nine hours, one meal, and much ice-water later, we had plotted and staked the last of 165 points and had connected them with twine.

Here, I think, we differed slightly from the Nazcas, for I seriously doubt they expended just over a mile of string (the total distance traversed by the outline). I rather suspect that they made their furrows (or at least preliminary scratched lines) as they progressed in plotting the various points. We could not do this, since rain threatened and would certainly obliterate our lines of powdered lime. But we did find it helpful (though not essential) to connect our points in sequence, to prevent possible confusion with stakes sometimes clustered rather closely together. (Otherwise we would have needed only a single long length of cord, to be used for the final marking of each straight line.)

The rains did come, and while no harm was done to our staked-out condor, large puddles (then more rain and still more puddles) prevented our completing our project for about a week. Finally, the ground had dried, the weather forecast was good, and the pilot was on standby. My father and I then spent much of one day marking the lines, finishing just in time to see the airplane circling.

Jerry Mays, a skilled local pilot, then took John and me up in his Cessna for a preliminary look and the taking of photographs, which John accomplished at just under 1,000 feet.

As Figure 2 shows, our work was a success. In fact the results were so accurate that we are convinced we could have easily produced a more symmetrical figure by this method. Thus it would seem—unless they employed an even simpler method of making the enlargement—that the Nazcas plotted considerably fewer points. That, coupled with mere visual estimation of right angles and less careful measurement (distances might simply be stepped off), could account for the imperfections we observed. Also, an entire small area, such as a foot, could have been done completely

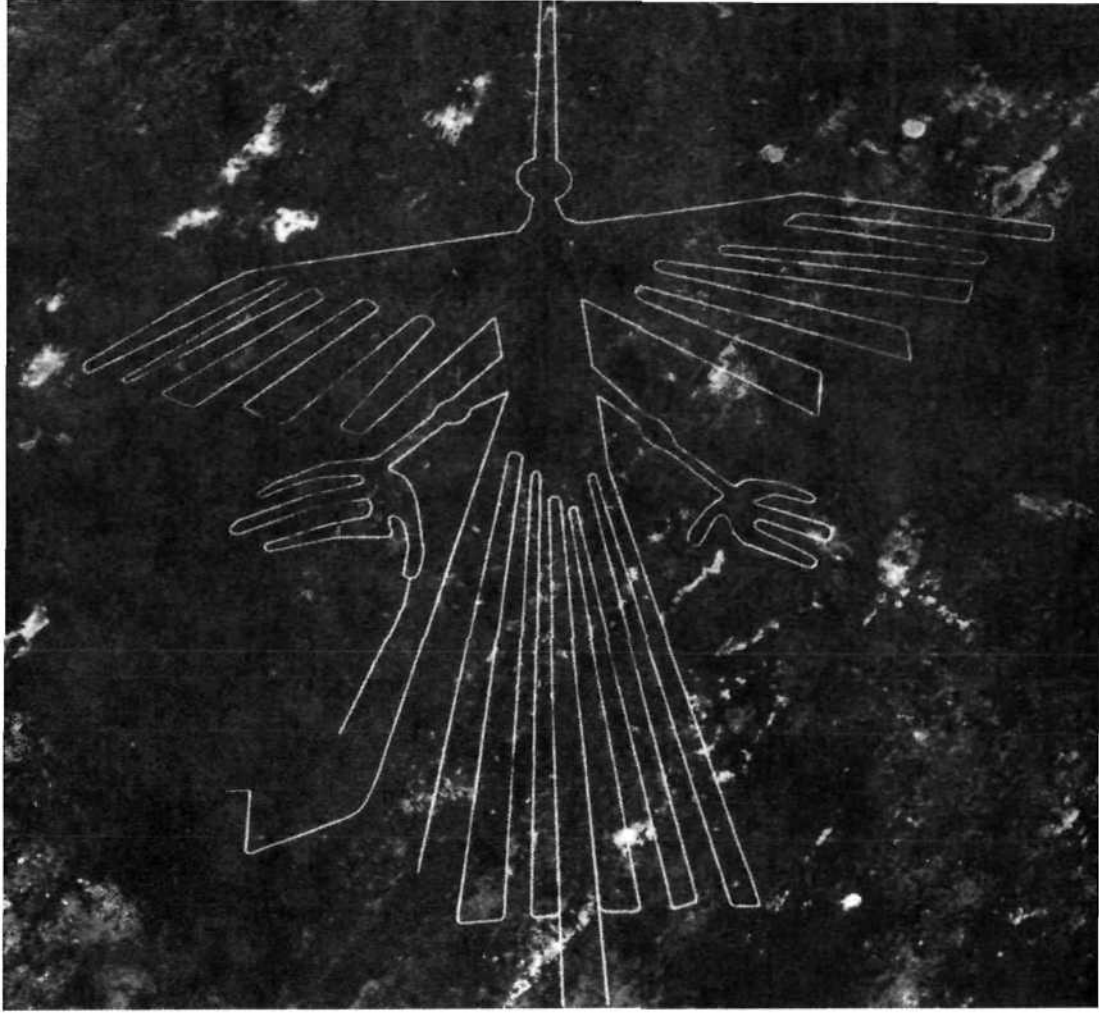

FIGURE 2. A duplication of the giant "condor" drawing made full size and utilizing only sticks and cord such as the Nazcas might have employed. The experimental drawing—possibly the world's largest art reproduction—is viewed here from just under 1,000 feet.

freehand. (Our own freehand work was minimal: We produced the circle of the head by scribing it with a rope. All other curves were *marked* freehand; of course we *had* plotted the numerous points that served as a guide, although we bypassed stakes slightly in attempting to draw smooth curves.)

It is frequently asserted that the Nazca drawings are recognizable only from the air. That is not quite true, certainly not of the smaller figures, such as the effigy of a fish, which is only 80 feet long (Reiche 1976). Neither is it true of some drawings—attributed to the Nazcas' predecessors—that are found on hill slopes (McIntyre 1975; Isbell 1978, 1980). Here, seemingly, is a clue to how the Nazcas could have been confident of the accuracy of their method of enlargement. Once a technique was found to be successful for

producing large drawings on slopes, where they could actually be viewed from the ground, the same technique could be expected to consistently yield good results—wherever figures were drawn and whatever their size.

Moreover, even the large drawings can be appreciated to some extent from the ground. With our condor, we were able to see whole portions—such as body and head, leg and foot, the entire fan of the tail—and thus had determined the figure was reasonably accurate even before our fly-over. We felt that an observer would be able to recognize it as a bird.

To test this possibility, my father took wildlife biologist Harold Barber to the site. Although Barber knew nothing of our project, and Nazca was deliberately not mentioned, on viewing the figure he recognized the drawing as one of the Nazca birds. That he was familiar with the Nazca ground drawings was unfortunate for our experiment (and rain prevented another); but the salient point is that he was able to identify the figure as a bird rather than as a spider, fish, monkey, or some other figure. In fact, when he was later shown pictures of several Nazca bird drawings, he immediately and correctly identified ours as the condor.

In summary, we do know that it was the Nazcas who produced the drawings. While their large size does suggest the possibility that they were meant to be viewed from above, as by the Indian gods, the figures can be recognized, at least to some extent, from the ground. The drawings could have been produced by a simple method requiring only materials available to South American Indians centuries ago. The Nazcas probably used a simplified form of this method, with perhaps a significant amount of the work being done freehand. There is no evidence that extraterrestrials were involved; but, if they were, one can only conclude that they seem to have used sticks and cord just as the Indians did.

Acknowledgments

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People's Almanac Predictions: Retrospective Check of Accuracy

The almanac's psychics aren't so perceptive

F. K. Donnelly

One of the most popular publications in recent years has been *The People's Almanac* series. These books make various best-seller lists and have sold millions of copies in their paperback editions. The successful formula employed by the editors is to present the reader with more than a thousand pages of entertaining and apparently accurate information. Subjects range from historical oddities to Hollywood scandals to sports records, to scientific data and geographical information about distant lands. Much of this information can be verified in other reference works. Yet in the introduction to the first *People's Almanac*, the editors declared their intention "to go beyond often repeated, unchallenged data and offer behind-the-scenes, frequently omitted *truths* [emphasis added]."[1] Let us examine this editorial objective with regard to the treatment of predictions by *The People's Almanac*.

The opening section of the first *People's Almanac*, published in 1975, offered readers some "Predictions by Present-Day Psychics."[2] Some of the past predictions of these psychics were set out according to whether they were right, wrong, or partly right. The clear impression given was that the psychics, while often wrong, were very often uncannily correct in their predictions of various political changes, natural disasters, scientific discoveries, and the like. Totaling the past records of 15 psychics as set out in this publication, we find that they were wrong 45 times, right 67 times, and partly right 7 times.

There are two problems in this arrangement of the past performances of psychics. First, we do not know the total number of predictions made by them and so we cannot be certain that the proportion of "right" scores has not been inflated by the elimination of vast numbers of "wrong" scores. Second, we cannot be sure that the so-called "right" predictions are not

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subsequent manipulations or reinterpretations of vague statements.

The People's Almanac of 1975 does, however, provide some data that enable us to surmount at least the latter problem. Each psychic was asked to record his or her predictions for the next few years. We can with hindsight now tabulate the results of their 1975 statements. (See Table 1.[3])

This is a rather dismal performance. Out of the total of 72 predictions, 66 (or 92 percent) were dead wrong. Among the favorites in this category were those that China would go to war with the United States (predicted 4 times) and that New York City would soon be underwater (predicted 3 times). My favorite inept prognostication comes from the Berkeley Psychic Institute, which predicted a war between Greenland and the Soviet Union over fish. Since nuclear weapons were to be used, this war was to be very sensibly fought in Labrador in May 1977. How this conflict involving NATO territory would not result in a larger war was not explained by the seven "experienced psychics" making up this "institute."

TABLE 1

Predictions of 21 Psychics
for period 1975-1981

Predictor	Right	Wrong	Partly Right/ Vague
Malcolm Bessent	0	2	0
David Bubar	1	3	0
Criswell	0	18	0
Joseph DeLouise	0	0	1
Jeane Dixon	0	4	0
Irene Hughes	0	1	0
Ann Jensen	2	3	0
Olof Jonsson	0	1	0
Bill Linn	0	2	0
Ethel J. Meyers	0	2	0
Dr. N	0	2	0
John Reeves	1	1	0
Shawn Robbins	0	9	1
Alan Vaughan	0	5	0
Berkeley Psychic Institute (7) experienced psychics)	0	13	0
Totals	4	66	2

This table is confined to predictions for the period up to the end of 1981.

If we turn to the four predictions that we have rather generously termed "right," we find that there is more trouble in store for the psychics. Two of the correct predictions involve simple projections from contemporary trends. We do not need to invoke a paranormal explanation to understand how John Reeves could predict that Russia and the United States would "remain as leading world powers" or how Ann Jensen could predict that there would be no world wars from 1975 to 1980. The latter involves what might be called the negation of the ultimate-catastrophe prediction. If there is no war, then Ms. Jensen is right. She would be wrong if there was a world war, but then copies of *The People's Almanac* might be difficult to locate amid the radioactive debris. Her 1975 prediction of an end to fighting in Vietnam by 1980 might also be explained as a nonpsychic estimate based on a knowledge of political trends.

We are left with just one prediction out of 72 that is at all intriguing. In 1975 David Bubar predicted for the period 1975-1980: "There will be mass murders, a kind of genocide, in South America." What immediately springs to mind is the mass suicide at Jonestown, Guyana, in November 1978. At the same time the vagueness of this prediction gives rise to doubts. The phrases in Bubar's statement might equally be applied to other events. Perhaps the actions of a repressive military junta might be interpreted as "a kind of genocide." An interview with a South American writer in the *Times* of London, for October 23, 1981, asserts that the persecution of journalists in Argentina must be seen as a "genocide."[4] To which case of a kind of genocide does Bubar's prediction apply?

Even if we were to accept that four (or 6 percent) of the 72 predictions were correct (by setting aside quibbles about random chance, vague wordings, and simple projections from existing trends), a further problem remains. Since we do not know which of the 72 predictions will fall into the 6-percent correct category, then of what use is this? Who among us would take the advice of a tipster with a track record of being wrong more than nine times out of ten?

In 1978 *The People's Almanac #2* was published, and again the first pages were devoted to predictions by psychics. For some reason only three of the psychics from the 1975 publication were consulted again.[5] Perhaps the discrepancy between, for example, Criswell's claim of 86 percent acccuracy on his predictions and his actual performance of zero for 18 was already becoming evident.[6] Did the editors then engage in an examination of the earlier set of predictions in their search for "the truth"? No. Instead, readers were presented with yet another list of prognostications by ten psychics who were given "past records" showing accurate predictions in more than two-thirds of their efforts.

When we come to examine the ability of this group to predict events of the period from 1979 to June 1982, we find a curious thing. (See Table 2.[7]) Once again their collective success rate is very low, only 4 percent. Nineteen of their 25 predictions were dead wrong, 1 was correct, and 5 were

TABLE 2

Predictions of 8 Psychics
for the period
1979 to June 1982

Predictor	Right	Wrong	Partly Right/ Vague
Frederick Davies	*0*	*1*	*1*
Maris De Long	0	1	0
Joseph DeLouise	0	1	0
Jeane Dixon	0	0	1
Irene Hughes	0	11	2
Daniel Logan	0	3	0
Jandolin Marks	1	1	1
Kathy Sotka	0	1	0
Totals	1	19	5

too vague to determine. Kathy Sotka's prediction that Anwar Sadat's "health will steadily decline, and he will be unable to perform political duties after 1980" has been classified as wrong. Sadat was assassinated on October 6, 1981, making both parts of her prediction wrong. At the same time this example shows how a selective "reinterpretation" of her statement would make it yet another "correct" prediction.

The People's Almanac #3 was published in 1981 and it had a surprise for its readers.[8] The traditional opening section on psychic predictions was eliminated without a word of explanation. Was this editorial decision the result of a learning experience and new insights about "the truth"?

In spite of the improved state of affairs at *The People's Almanac*, there can be little doubt that otherwise rational people will continue to believe in, accept, and even pay money to hear the plain guff spewed out by "psychics." Accordingly, I want to propose a simple and straightforward method for dealing with psychic prognosticators. My proposal is a simple one, since it does not involve rational argument or a knowledge of statistical inquiry.

A foundation would be set up in Las Vegas with a small amount of seed money to place bets on formal predictions made by psychics. This foundation would issue direct challenges to prognosticators to put their money where their collective mouths are. With the odds running at more than nine to one against the psychics, we would soon have a well-endowed research foundation (and a number of impoverished crystal-ball gazers).

Consider the certainty involved in betting against the fulfillment of the following predictions by Criswell:[9]

> In 1982, a dying planet named Bullanon will come so close to earth that it will affect the earth's gravity, shifting the poles. This will cause the rising of a lost continent . . . and trigger earthquakes, the destruction of cities, eruption of volcanoes, storms, and tidal waves.
>
> Between February 11 and May 11, 1983, all the women in St. Louis will lose their hair.
>
> In 1985, polygamy will become legal in North Dakota.

Do we have any takers?

Notes

1. David Wallechinksy and Irving Wallace, eds., *The People's Almanac* (Garden City, N.Y.: Doubleday, 1975), p. xv.
2. Pages 1-11 were written by Ann Elwood and Irving Wallace.
3. Robert McLintic's predictions have been deleted because they are undated.
4. George Brock, "Remember the writers put behind bars," *Times*, London, Oct. 23, 1981.
5. David Wallechinsky and Irving Wallace, eds., *The People's Almanac #2* (New York: Bantam, 1978). Pages 1-8 and the psychic-prediction section were written by Richard De A'Morelli and Ann Elwood. Only Joseph DeLouise, Jeane Dixon, and Irene Hughes have predictions in both volumes.
6. See the first *People's Almanac* (1975), p. 2, for Criswell's claims of his accuracy and his knowledge of "the future through the year 1999." For the performance of another psychic washout, see James Randi, "'Superpsychic' Vaughan: Claims vs. the Record," SKEPTICAL INQUIRER, Summer 1981, pp. 19-21.
7. The undated predictions of Tenny Hale and Kebrina Kincade have been deleted from this tabulation.
8. David Wallechinsky and Irving Wallace, eds., *The People's Almanac #3* (New York: Bantam, 1981).
9. *The People's Almanac* (1975), p. 3. •

The Chance to Convey Science's Pleasures

Some scientists seem unwilling to engage in public confrontations on borderline science issues because of the effort required and the possibility that they will be perceived to lose a public debate. But it is an excellent opportunity to show how science works at its murkier borders, and also a way to convey something of its power as well as its pleasures.

—Carl Sagan in *Broca's Brain*

A Test of Numerology

How well do numerological descriptions for preferred names compare with self-perception of personality?

Joseph G. Dlhopolsky

Numerology is the study of the occult significance of numbers, according to Webster. Numerologists claim that a person's name, when converted to numbers, reveals a wealth of information about that individual's personality, past, and destiny.

From the standpoint of intent, the practice of numerology does not differ from techniques like fortune telling, astrology, tea-leaf reading, palmistry, and tarot cards; all attempt to tell us something about our futures through arcane means. However, these tecnhiques share another characteristic as well: they all search for personality attributes and destiny by relating them to patterns in nature or chance that can more or less arbitrarily be assigned to an individual. These patterns might be the position of the planets at birth, the configuration of crease lines on the palms of the hands, a random sequence of cards dealt in a reading, or, as in the case of numerology, the letters that make up one's given name, the name one chooses to use, and one's birthdate.

As part of a seminar that explored the evidence for and against paranormal phenomena, my students and I carried out a small-scale investigation of numerology. This study was conducted in conjunction with a demonstration of numerological techniques by a former student. The student, Michelle Mudry, claims a longtime interest in several occult areas, including numerology. Her presentation is the source of much of the numerological information described here.

There are two major numerological systems in general use. They are the "modern" system and an ancient "Hebrew" system. The technique for arriving at significant numbers is the same for both, but the actual numbers

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assigned to letters of the alphabet is different in each. In any event, to perform a numerological workup, the subject is asked to write his or her full name as given at birth and the name by which he or she is now known or prefers to be called. The name that one chooses to use is believed to express a person's current personality characteristics, while the name given at birth more fully describes one's destiny. In both systems, the letters of the name are assigned digits according to the system in use (Table 1). For

TABLE 1: The two major systems used for assigning digits to letters of a subject's name. Note that the Hebrew system, while using the digits 1 to 8, can still arrive at a name number of 9 (the partial sum of 18, for example, gives a result of 9).

Modern System

Number	1	2	3	4	5	6	7	8	9
	A	B	C	D	E	F	G	H	I
	J	K	L	M	N	O	P	Q	R
	S	T	U	V	W	X	Y	Z	—

Hebrew System

Number	1	2	3	4	5	6	7	8
	A	B	C	D	E	U	O	F
	I	K	G	M	H	V	Z	P
	Q	R	L	T	N	W	—	—
	J	—	S	—	—	X	—	—
	Y	—	—	—	—	—	—	—

each name, given and preferred, the digits are added together, the result being a two-digit number for each. The digits comprising this number are then added together so that one single digit remains for each name. For example, the name Joe Smith would, according to the modern system, consist of these digits: 165 14928. These would reduce to 36 and then to 9. The digit 9 is believed to hold a significance for Joe Smith different from that for individuals having one of the other eight digits, but similar to that for other 9s. Moreover, if Smith chose to change his name slightly or completely (as would a woman getting married and adopting her husband's surname), his personality would show a change if his name number changed with the new name. However, his given name, Joseph

Smith III, would not change, nor would the number derived from it.

Obviously, the total number of personality patterns that are possible in both systems is not enough to differentiate between the varieties of personality, there being only 81 combinations of given- and preferred-name numbers. So added complexity is achieved by calculating additional significant numbers. The number of consonants and the number of vowels in the name yield their own numbers. The birthdate comes into play as well, being directly reduced to a single digit in the same way. The resulting digits, which range from 1 to 9, are used singly and in various combinations to convey what is believed to be accurate information regarding the subject's *inner personal outer persona, and destiny.*

For purposes of the class demonstration, the modern system was used and Mudry identified only the numbers for the subjects' preferred names. Names were supplied by each of the ten students in the class and by me, the instructor. While a complete numerological workup for each student was not possible or feasible under the circumstances, some positive indication should have surfaced if numerology actually works.

Mudry used popular numerology references to prepare nine cards bearing personality descriptions for each of the possible name numbers. The plan was to have each subject evaluate the descriptions for each number before his or her actual name number was revealed. Presumably, since the subjects presented their names as they preferred to use them, the resulting name-number descriptions should have demonstrated some agreement with the students' self-perceptions. In an attempt to evaluate the extent of this agreement, each student rated each of the nine descriptions

TABLE 2: Raw data for each of the possible name-numbers. The figures with asterisks (*) under each subject are the ratings given by the subject for his or her actual-name number.

Subject	1	2	3	4	5	6	7	8	9	10	11	Mean
Number												
1	2	2	3	3	4	5	2*	4	5*	2	2	3.1
2	3*	3*	2	3	2	1.5	4.5	2	2	1.5	5	2.7
3	2	1	3	2	3	2	2	2*	5	2	2	2.4
4	3	5	5	5	5*	5	5	5	5	5	5	4.8
5	4	4	5	3	2	2	3	4	2	5	2	3.3
6	4	2	2	2*	2	4	5	3	4	4	4*	3.3
7	4	1	5*	3	5	4	2	4	3	3	5	3.5
8	5	2	4	3	5	2*	5	5	3	3*	1	3.4
9	5	2	4	2	3	1	3	3	1	2	1	2.4
Mean	3.6	2.4	3.7	2.9	3.4	2.9	3.5	3.6	3.3	3.0	3.0	3.2

on a scale of 1 to 5, with the responses indicating the following: 1 = description very close to self-perception; 2 = moderately close; 3 = neutral; 4 = moderately wrong; 5 = very wrong. The data appear in Table 2.

The overall rating for all nine name-numbers was 3.2, which corresponds closely to a neutral rating. This might be expected to occur if eight out of the nine numbers did indeed fail to describe one's perceived personality and destiny. However, this value remained at 3.2 when the ratings for the name numbers were considered apart from the other numbers. The reason for this was that the mean rating for the name numbers was only 3.3. A two-tailed *t*-test for correlated scores was performed on the subject's name-number ratings and mean non-name-number ratings. Not surprisingly (considering the small differences), the results were not significant (t (10) = .142, p = .88).

Overall, the results do not leave one with the impression that numerological descriptions for preferred names compare well with the self-perceptions we hold for our personalities. This contrasts with the reported amazement of "believers" when, by their interpretation, something startling and obviously true is revealed through the medium of astrology, fortune telling, or the like.

While the numerological hypothesis was not supported in this study, one should be cautioned that the experiment only looked at one facet of numerology, that of preferred-name numbers. Clearly, there is a need in this area for more empirical data from a greater number of subjects to compare with the wealth of anecdotal evidence. ●

Pseudoscience and Lost Opportunity

The danger of pseudoscience is not to science, but to society, to democracy and to the personality. . . . It is primarily an opportunity cost, in the language of economics. Here we have all these people interested in the exciting and wonderful and amazing properties of the world who could be learning something real. Instead they are hearing from Uri Geller. They've turned themselves over to dependence on charlatans. They could be learning something different.

—Philip Morrison, quoted in "Will the Real Science Please Stand Up," by Kendrick Frazier, in *SciQuest* (American Chemical Society), September 1981.

Pseudoscience in the Name of the University

What should be done about extension courses that use the university's prestige to promote pseudoscience?

Roger J. Lederer and Barry Singer

In recent years the teaching of pseudocience and quackery in universities has become common and apparently accepted under the aegis of academic freedom. Typically this material is not formally presented as "Pseudoscience 101," but is offered as a component of a regular course. To cite a few examples from our own experience: an instructor in Mexican-American studies holds that, since the Aztecs could not have built their empire without paranormal communication, a course in Chicano history should consist of ESP training; an educational-psychology instructor pushes parapsychology as the new hope for education; a course in "Law, Psychology, and Consciousness," taught in the Finance Department at Cal State, Long Beach, maintains that, if infants were born using the Leboyer method, world peace would be achieved; a Community Service course at Cal State, Chico, makes the statement that manipulation of cranial bones can straighten teeth.

Pseudoscience is even more in evidence in extension courses. This article discusses the sorts of professional monitoring that seem ethically and practically appropriate.

There are few instances where there has been direct peer or administrative intervention to quash the teaching of such nonsense. Although such instances exist, in the great majority of cases there is a "hands-off" attitude. The rationale for nonintervention is, apparently, academic freedom, and might be stated as follows: "Incompetence and the teaching of fringe ideas is part of the price of academic freedom. If we were to bear down on the teaching of pseudoscience, we would set in motion a surveillance and enforcement apparatus that would be difficult to contain.

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Such an apparatus might then be used to repress any currently unpopular idea. Adequate opportunities exist to rebut the pseudoscience of errant colleagues by speaking out in our own classroom and in public forums." This is undeniably a powerful argument, but we believe that the counterarguments are even more compelling.

Every professor has a general concept of what academic freedom means and is supportive of it, but would probably be hard-put to construct a detailed explication. A search of administrative files in most universities might well reveal little or no literature on academic freedom.

Academic freedom in its present form is a relatively recent development, conceived by the nascent American Association of University Professors at the turn of the century. It has never been a legal concept. By and large the courts will not recognize or enforce it, except insofar as it coincides with First Amendment privileges. Academic freedom's force derives from such enforcement as the AAUP can muster and from the regard it receives from the academic community. Academic freedom is primarily intended as a bulwark against administrative retribution for the professing of socially and politically unpopular positions. Thus a typical academic-freedom case might involve the suspension of a biology professor who makes a public statement in favor of premarital sex or against the Vietnam war.[1] Although relevant cases may exist, in a telephone conversation the AAUP staff could not recall an academic-freedom case where the surface issue was incompetence or the teaching of nonsense.

Most important, academic freedom does not give professors the license to teach whatever they personally believe to be sound. Professors are answerable to a broader context of professional standards. Thus to move against the teaching of astrology would not be a violation of academic freedom. The AAUP has repeatedly been absolutely clear and firm on these points.[2] One recent statement of academic freedom reads, " . . . academic employment, protected by academic freedom, requires competence and the waiver of the exercise of the constitutional rights to foolishness, ignorance, or deceptiveness . . ."[3] Academic freedom thus does not give one the right to teach pseudoscience.

The arguments for moving against colleagues who teach pseudoscience are several. Such teaching practices discredit the profession and the university. They may be harmful either in a general way, for example, if the student is thereby confused about the validity of science and habits of logical thought; or directly, for example, if the student is taught to take Laetrile to cure cancer, or to practice faith healings. These are not trifling or hypothetical examples. Both the professor and the university would be vulnerable to a lawsuit brought by a student whose health has been damaged. To the extent that there is actual classroom proselytizing for a questionable idea or technique, the professor is again in violation of academic freedom tenets as well as legally liable. To the extent that the

course substance does not match the catalogue description, the professor is in violation of academic policy and is not protected by academic freedom. Finally, statements of academic-freedom concepts quite clearly limit such freedoms to a professor's area of competence. Professors of English or history may be persuaded that scientists are biased on the UFO question, but they do not have the right to express such views in their classrooms.

Those of us in academia are often critical of other professionals, such as doctors and lawyers, for being reluctant to police themselves. Yet we have done nothing whatsoever in that regard. Gullible students and the public are being told falsehoods by our colleagues. Belief in these falsehoods is no doubt facilitated by public trust that, if these ideas were outright nonsense or fraud, the university would not permit their teaching. We are lending public forums and academic authority to pseudoscience and nonsense, and have acted so far as if we had no responsibility in the matter.

We are perhaps intimidated by the aggressive intellectual posture of pseudosciences. If a history professor were to teach that the United States fought a war with the Belgian Congo in 1790 and a peace treaty called the Warner Act was subsequently enacted, the History Department would, we hope, have a talk with him. Yet even more ridiculous ideas are left untouched as "alternative points of view," perhaps because there are books written to support them, societies and newsletters centered around them, and so on.

Pseudoscience courses in extension or continuing education raise different and more severe problems. We shall give only a few among many examples. In 1980, the University of California at Santa Cruz extension offered a course called "The Human Aura." According to the university's course announcement, the faculty included Valerie Hunt, who had developed "high frequency electronics by which the human aura, in all its

various colors, can be detected, measured, and recorded," the Reverend Rosalynn Bruyere, who "utilizes her ability to see auras in great detail to assist in detecting variations in auras which may indicate the onset or presence of disease," and Ms. Emilie Da'oud, who "has demonstrated the capacity to change the color of her aura at will and make it disappear." Among the healing skills to be imparted during the course were: the ability "to see in color inside the body" and to "transmit healing energy by the laying on of hands," "the use of sound and movement to produce frequencies which seem to facilitate paranormal healing," and "direct mind-to-mind contact to eliminate blocks to psychic healing." Powerful stuff. This course was approved for continuing education for nurses and pharmacists.

For years UC-Santa Barbara has sponsored extension courses on "Spiritual Hypnosis," in which Dr. Peter Francuch probes multiple consciounesses in numerous physical dimensions and contacts spirit guides therein who counsel on mental disorder. He learned the technique through communion with his own spirit guides. UC-Berkeley sponsored a symposium on "Jungian, Transpersonal, New Age Residential Treatment Techniques" that demonstrate the "central importance and transformative power of archetypes in the healing of adolescent schizophrenia." Some of the courses above were certified as meeting continuing-education credits for therapists and counselors.

In November of 1980, the profligacy of such extension courses in the California university system was brought to the attention of George O. Abell, professor of astronomy at UCLA. Abell wrote a detailed, strong letter of protest to the statewide faculty committee on extension. The committee recognized the problem as serious and issued a policy statement urging close monitoring of extension courses by the regular faculty and directly discouraging courses that presented pseudoscience. However, the state committee has neither a surveillance nor an enforcement function, and the problem has not noticeably abated, except at UCLA. Even in a state system, extension programs are autonomous, answerable only at the local level. In the past several months we have received flyers advertising a course titled "Altered States of Consciousness, Paranormal Phenomena, and Human Potentials" at UC-Santa Barbara; a conference on holistic education offered for professional upgrading credit, at UC-San Diego, whose varied program advertises "visual inner work to harness our inner sanctuary," "left/right brain integration in flotation tanks" as an "exciting new teaching tool," acupressure, and "breath awareness and rebirthing practices that effect amazing and magical transformations." UC-San Diego has also recently sponsored a conference on using visualization to cure cancer, and offers relicensure credit therein for nurses and social workers. The flyer states that "the psyche and emotion may play a critical role in the development of cancer and therefore hold the key to the treatment of the disease . . . The findings to date indicate that many

patients using this method . . . significantly outlive their life expectancy and report improvements in the quality of their existence." UC-San Diego has offered continuing courses in "past-lives therapy," available for professional relicensure credit for nurses, social workers, and psychologists, which claim that "mounting evidence from this therapy indicates that traumatic events . . . may be carried over from our past lives, thus affecting our present behavior and experience. . . . Exploring the images of what seem to be previous incarnations" will fix this problem.

All of this would be amusing were it not so shameful and sad. Although we are unfamiliar with extension offerings elsewhere, we are sure that this problem is not confined to California. If regular university faculty were to scrutinize their extension catalogues, they might find some unpleasant surprises. The reasons for this problem are several. First, extension divisions generally must generate all of their own revenue. Pseudoscience courses are exceptionally popular; they generate revenues that can underwrite more respectable but less popular courses. Second, although most universities have a policy that extension courses are to be screened by the regular faculty in the appropriate discipline, in practice the regular faculty seldom have the time or interest. To do an adequate job, a regular faculty member must be granted released time through extension funds. This practice would raise extension fees, which are already too high for low-income people. Third, extension personnel and deans do not typically have the expertise themselves to do a screening and may be deluded into thinking, for instance, that someone who is a "Certified Touch-for-Health Instructor" possesses useful skills. Fourth, in a number of states the legislature and professional associations have mandated a minimum number of approved extension units per year for relicensure or professional upgrading. The professional associations in theory retain the right to certify the courses that will be approved for professional credit. In practice, however, they delegate that right in a carte-blanche fashion to extension programs, with the disastrous results outlined above.

A single such course offered by a prominent university outweighs a year's worth of CSICOP activity and SKEPTICAL INQUIRERS. The brochures advertising such courses display the name of the university prominently. They are colorful, glossy, and slick, and are distributed widely in the community. "Here," they say, "is our latest and best. Come sip from the advancing ripples of the fountain of knowledge. Our imprimatur is upon this course." Certification for professional credit is advertised in the brochures. Typically such brochures contain not a hint of a disclaimer. It would be of little use if they did. The public impression is that of exciting new knowledge, certified and heavily promoted in the name of an important university. If a regular faculty member at such a university were to critique pseudoscience, the public would rightly perceive him as an absolute, cranky minority, out of step with what his own university is loudly declaiming. The public does not perceive that different

standards and different faculty are to be found in extension offerings. The "faculty" who teach such classes then widely list and promote themselves as, for instance, "Lecturer, UCLA" The public assumes that the university has certified the credentials of the person teaching the course.

A related problem is the holding of various organizational conferences at universities. As with extension courses, universities use conferences to generate revenue. Organizations essentially rent university facilities and resources to hold their conferences. However, their promotional literature typically reads "sponsored by" the university, and the university name is displayed prominently in the flyers. Once again, it is doubtful that the public discriminates between "renting of facilities" and "sponsorship" by the university. The University of California system recently refused to allow a neo-Nazi group to hold a conference in its facilities, for ethical reasons. However, Cal State-Long Beach was pleased this year to "sponsor" and widely promote a regional Rosicrucian conference, whose conference brochures advertised auras, telepathy, and the whole paranormal grab-bag.

If universities continue to abuse the people's trust, at some point there will be an adverse reaction from an awakened public. If we continue to tolerate the teaching of pseudoscience by our colleagues in the regular curriculum, justifying our inaction in the name of academic freedom, that precious privilege will acquire a very bad name. If extension programs in our prestigious universities continue to gull a trusting public, aggressively hawking and windowdressing nonsense as do the worst commercial publishers in order to generate revenue, the public will someday call those universities to account. The process of generating revenues to support our mission is gravely undermining that mission, perpetrating fraud, and abusing public respect and trust.

Notes

1. H.W. Baache, ed., *Academic Freedom* (New York: Oceana, 1964); S. Hook, *Academic Freedom and Academic Anarchy* (New York: Cowles, 1970).

2. Personal communications, Jonathan Knight, AAUP, One DuPont Circle, Washington, D.C. 20036.

3. H.W. Baache, op. cit., p. 86. •

Book Reviews

Psychology of the Extraordinary

Anomalistic Psychology: A Study of Extraordinary Phenomena of Behavior and Experience. By Leonard Zusne and Warren H. Jones. Lawrence Erlbaum Associates, 365 Broadway, Hillsdale, NJ 07642, 1982. 498 pp. $29.95.

Reviewed by Victor A. Benassi

Anomalistic Psychology is a detailed investigation into psychological explanations of allegedly paranormal behavior. It reads like an introductory psychology textbook. In fact, the premise of the authors, psychologists Leonard Zusne and Warren Jones, is that we can go a long way in accounting for what appear to be paranormal phenomena through the judicious application of standard psychological principles. The analyses are generally so incisive that paranormal claim after claim vanishes as if in a disappearance act. The book tells us a great deal not only about anomalistic phenomena but also about standard psychological topics—e.g., perception, physiology, memory, belief formation and change, group processes, learning, quantitative methods, abnormal behavior, and personality.

The authors, as expected, cover familiar ground (ESP, spiritualism, faith healing, poltergeists, occultism, miracles). They also expound on exotic-sounding topics (inedia, pareidolia, dermal vision, glossolalia, automatic writing). While the sections of the book that gently debunk paranormal claims are interesting and stand in stark contrast to C.E.M. Hansel's "gun 'em down" approach, the really fascinating parts of the book are those that seek to account for the development and maintenance of belief in various anomalistic phenomena. Equally brilliant are the discussions of how heroic anomalistic feats like "speaking in tongues" might be explained without recourse to paranormal accounts.

While Zusne and Jones are not the originators of many of the analyses and ideas they present, they demonstrate a penchant for synthesizing other scholars'

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work. However, *Anomalistic Psychology* is not simply a distillation of what is already known. Two of their original contributions come easily to mind: the conceptualization of extrasensory perception within the framework of signal-detection theory and the authors' empirical work on personality correlates of paranormal beliefs.

Although the coverage is generally excellent, I found several noteworthy omissions in the book. While two chapters are devoted to extrasensory perception, there is no discussion of the work by physicists Puthoff and Targ on remote viewing. I mention this because one of their original articles was published in a journal of international reputation, *Nature*. The story behind the decision of *Nature*'s editor to publish this article* and the subsequent scientific squabble about remote viewing, which has not yet abated, are alone adequate reasons why readers should be familiar with Puthoff and Targ. Additionally, the book only indirectly discusses recent research on judgmental biases and heuristics. Several authors (e.g., Alcock, Kammann, and Singer and Benassi) have suggested that human shortcomings in reasoning may contribute to a variety of paranormal beliefs.

Still, I am hard-pressed to find glaring omissions. *Anomalistic Psychology* aims at being comprehensive, and it succeeds. In my opinion, no other book that both examines the evidence for and against various paranormal claims and develops a psychology of paranormal belief is as successful as this one (compare Alcock. *Parapsychology: Science or Magic?* 1981; Marks and Kammann, *The Psychology of the Psychic*, 1980; Neher, *The Psychology of Transcendence*, 1980). Not that these other books are of poor quality. Quite the contrary. Rather, *Anomalistic Psychology* is set apart because it does in a comprehensive fashion what these other books do with a more focused subject matter. The complementary backgrounds of the authors no doubt contributed to their fine effort. Zusne works in the areas of perception and the history of psychology. Jones is an active researcher of theoretical problems in social/personality psychology. Both authors have done original empirical work on various aspects of anomalistic psychology.

Along with an increasing interest in the paranormal among people in the lay and scientific communities, there has been a similar increase in course offerings that provide critical perspectives on the paranormal (for a list of such courses write to Leonard Zusne, Department of Psychology, University of Tulsa). I will definitely make *Anomalistic Psychology* the central text in the course I teach. If there was a shorter version, I would consider using it as a supplemental reader in my introductory psychology course. While the text has a secure place in the academic market, it could also be read by educated lay readers, although it would not be light reading.

In sum, Zusne and Jones have set an impressive standard of scholarship for other writers on the paranormal. It is my hope, and I'm sure that Zusne and Jones agree, that future books on the topic will do them one better. The task will not be easy. ●

*Benassi, V.A. "Scientific Progress in Psi?" Book Review of C.T. Tart, H.E. Puthoff, and R. Targ, *Mind at Large* (IEEE symposia on the nature of extrasensory perception), in *Contemporary Psychology*, 1982, 27(7), 548.

Stars, Planets, and People

Astrology: Science or Superstition? By H. J. Eysenck and D. K. B. Nias, St. Martin's Press, New York, 1982. xi + 244 pp. $12.95.

Reviewed by Paul Kurtz

H. J. Eysenck, noted University of London psychologist and one of the coauthors of *Astrology: Science or Superstition,* is well known for his defense of controversial theories and his willingness to jump in to defend unpopular claims even before they are thoroughly tested. As a student of Cyril Burt, he was a longtime defender of Burt's work on I.Q.'s, though, in one of the scandals of contemporary science, it was shown that Burt apparently fudged the data of some of his studies of twins. Eysenck was also an early outspoken proponent of Jensen's thesis that there are racial differences in I.Q. Martin Gardner has pointed out that Eysenck also loudly defended the thesis, based upon his (Eysenck's) research, supported by the tobacco industry, that there is no linkage between cigarette smoking and lung cancer.

In recent years, Eysenck's role in astrology has had some impact not only in scientific and quasi-scientific journals but also in the mass media. In 1978, he published a study (along with Mayo and White) that allegedly tested the relationship between personality and sun signs.

Astrology claims to predict the two major dimensions of personality: emotional stability and introversion/extroversion. According to astrology, the signs are either positive (odd-numbered), denoting masculine, outgoing qualities, or negative (even-numbered), associated with feminine passivity and self-repression. Similarly, earth signs denote practicality and stability, and water signs, the intuitive and emotional.

Eysenck, Mayo, and White tested 2,324 subjects. The results were hailed by Eysenck and company as "striking." Indeed, the astrological journal *Phenomena* lauded the study as "possibly the most important development for astrology in this century." Critics have pointed out a major flaw in the study: most of the subjects questioned already had prior knowledge of astrology, which may have influenced the results. (See *SI,* Summer 1981.) Eysenck now concedes that he was overhasty in his claim He admits that those who had some knowledge of their signs were more likely to score positively than those who did not. Repeated efforts to replicate the results have failed. Those who are ignorant of astrological characteristics do *not* show any tendency to match their personalities with those predicted by astrology. "All this should teach us to be cautious," he says. "The lesson is that we must test for alternative explanations before we accept any result . . . as a confirmation of astrology" (pp. 51-60).

I raise these points because Eysenck has emerged as possibly the leading scientific supporter of the work of Michel and Francoise Gauquelin. He condemns those who have raised serious questions about the Gauquelins' work. Although Eysenck and Nias are themselves highly critical of the claims of popular and traditional astrology, in *Astrology: Science or Superstition* they maintain that the Gauquelins are contributing significantly to the new science of "cosmobiology"

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and that this tends to support somewhat the basic premise of astrology: that there is a relationship between heavenly bodies and personality.

Eysenck and Nias open their book with a strong condemnation of skeptics. They accuse them of being closed-minded and dogmatic. In particular, they attack the 186 scientists who endorsed "Objections to Astrology," a statement published in the *Humanist* in 1976 when I was its editor, and I agree with them that one should avoid any "appeal to authority," which would be fallacious. Eysenck and Nias concede that, in so far as the anti-astrology statement was directed against popular astrology, they "fully agree with it; popular astrology is nonsense, and the newspapers and magazines which print it should be thoroughly ashamed" (p. 211). However, they accuse the scientists who signed the statement of not having investigated the evidence, though they themselves admit: "we do not claim to be experts in astrology, and we could not interpret a birth chart with any degree of confidence . . . What is important is the ability to look at the design of the various studies with a critical eye, and the statistical knowledge to examine the results obtained" (p. 13). I submit that the same thing may be said to apply to many or most of the scientists who endorsed the statement.

I would like to point out that the statement was intended primarily as a criticism of popular astrology and that, until it was released, there had been virtually no open scientific criticism of astrology. The statement was intended to alert the public—29 percent of whom, according to a Gallup poll, believed in astrology and had been largely influenced by its overwhelmingly favorable treatment in the media—to the fact that there is a substantial amount of evidence against astrology. The most important effect of the statement was that it stimulated a dialogue on the merits and demerits of astrological claims and helped to provoke further scientific inquiry, which, in the last analysis, is the only way to deal with astrology. I agree with Eysenck and Nias: only the facts can determine its truth or falsity.

Where does *Astrology: Science or Superstition?* stand in regard to the truth or falsity of astrology? Eysenck and Nias present some telling criticisms of popular and traditional astrology and show why it fails. Much of this should be applauded by hard-nosed skeptics. Although this book is not as detailed as Dean and Mather's excellent work, *Recent Advances in Natal Astrology* (1977), which thoroughly surveys the research in this field, the authors show that horoscopes are so general that they can be interpreted in many contradictory ways by astrologers, that astrological charts are usually framed in positive terms so that they tend to be rated as highly accurate by individuals, even when the chart being evaluated is actually one that was drawn up for someone born at a different time and place. The authors also point out serious astronomic difficulties with astrology: though astrology still draws on traditional formulations, the precession of the equinoxes has skewed the sun signs by 30°; the outer planets were unknown to early astrologers; and so on.

Eysenck and Nias ask why so many astrologers claim a high success rate in their work. They submit that it is because the criteria for assessing astrological predictions are too vague; gullible subjects tend to read into them what they want. Astrology functions in a quasi-religious way. They also point out that Indian, Chinese, and Western astrologers all claim to have high success rates yet use radically different systems. When astrological predictions appear to be accurate, it may be due to self-fulfilling prophecies, i.e., people who already believe in astrology tend to certify it by confirming those characteristics it attributes to them.

Presumably this is what happened in the test by Eysenck, Mayo, and White discussed earlier.

Eysenck and Nias review efforts to submit astrology to scientific evaluation: they conclude that there has not been adequate empirical testing to confirm it. The main problem is that there have been inadequate experimental designs or insufficiently large samples and no replication of any claimed findings. Interestingly, they often quote the Gauquelins, who are critics of popular and traditional astrology—particularly horoscopes. In one famous test, Michel Gauquelin placed an advertisement in *Ici-Paris* offering a personal horoscope free. He sent everyone the horoscope of France's Dr. Petiot, a notorious mass murderer. Of the first 150 replies, 94 percent of the recipients said that the false horoscope accurately described their characters.

Eysenck and Nias review a number of other interesting claims. They are critical of the work of John Nelson on planetary magnetic disturbances on earth. Quoting from the work of Geoffrey Dean, they agree that there is "not the slightest support for Nelson's claims" (p. 157). They are highly critical of sunspot theory. Similarly, they attack Gribbin and Plagemann for their predictions of the "Jupiter Effect." "Provisionally at least," they say, "we must conclude that planetary forces do not seem to have any clearly apparent effect either on the sun or directly on the surface of the earth" (p. 162). They also deny that there is any real factual support for lunar influences.

Concerning traditional and popular astrology, Eysenck and Nias conclude: "In terms of the question posed by the title of this book, we therefore conclude that astrology is largely (but not entirely) superstition. However, because of the important areas which remain to be investigated, this conclusion may need future qualification. We should not be dogmatic. . . ." The general inference they draw is: "The major failure to which we have returned time after time is the lack of replication" (pp. 213-14).

"Cosmobiology," however, is a different matter. "If, in judging classical or traditional astrology, we have had to side with those scientists who find little good to say about it, the position is rather different when we turn to cosmobiology" (p. 218). Cosmobiology refers to the work of the Gauquelins, in which the claim is made that there are strong statistical correlations between highly successful people (their personality types) and certain planetary positions (Mars and sports champions, or the so-called Mars effect; Jupiter and generals; Saturn and scientists; etc.). Critics of the Gauquelins point out that their hypotheses have not been sufficiently confirmed. Efforts to replicate the results of a sample selected by the Belgian Comité Para have led to interminable disputes about the interpretation of the results. Abell, Zelen, and Kurtz studied 408 sports champions in the United States and found no support for the "Mars Effect." This interpretation has been disputed by Gauquelin, and some critics have faulted the way the tests were conducted. Some of the criticisms are based upon hearsay, gossip, and wounded egos. Some of them, in retrospect, I believe are warranted. But I submit that if these criticisms are valid, then they can be equally raised against the Gauquelin research as well. For the Gauquelins did not allow for sufficient safeguards or controls. The point is that, thus far, the Gauquelins' theories have *only* been established by the Gauquelins using *their own data*. It was the Gauquelins, for example, who sent for the birth-time data for the Belgian Comite Para sample.

Thus there still has not been an entirely independent replication. Until there is,

skeptics are unwilling to agree that the Gauquelins' hypotheses have been proven. Eysenck and Nias maintain that they "can find no valid major criticism of [the Gauquelins'] conclusions, methods, or statistics" (p. 219). However, they themselves admit: "The only fault we have to find with them is simply that they have not been replicated; we have made replication the corner-stone of what is and what is not properly acceptable to science" (p. 221). But if that is the case, how and why can Eysenck and Nias affirm that they are sympathetic to or willing to accept the findings of the Gauquelins? Both believers and skeptics, those sympathetic to or neutral concerning astrology, it seems to me, should agree on the need for new, unbiased double-blind tests of the Gauquelins' hypotheses. Eysenck and Nias quote Mather: "It is probably not putting it too strongly to say that everything hangs on it. If Gauquelin's results are ever shown to be spurious then, relatively speaking, the positive evidence that remains for astrology is weak" (p. 209). Yet they then go on to say that Gauquelin has found something, and they conclude their book by asserting: "Perhaps the time has come to state quite unequivocally that a new science is in process of being born. Amid all the dross, there does seem to have been a nugget of gold" (p. 209).

To which I respond, the evidence is by no means all in. Efforts at clear-cut independent replications until now have failed. Until there are additional ones by those other than the Gauquelins, we have a right to ask whether still another self-fulfilling prophecy is at work. Are Eysenck and other followers of Gauquelin correct when they maintain that the Gauquelins have made or are on the verge of making a major scientific breakthrough? Perhaps it only depends upon one's bias. I would hope not. But is not the proper moral to be drawn that we need to have additional replications of the Gauquelins' claims before we can accept the reality of "cosmobiology," let alone astrology? •

A Passion for Truth

Science: Good, Bad and Bogus. By Martin Gardner. Prometheus Books, Buffalo, New York, 1981, 412 pages, $18.95, cloth. Avon, New York, 1983, 408 pp., $3.95, paper.

Reviewed by Michael R. Dennett

Martin Gardner has been a conspicuous participant in CSICOP since the group's inception, and his latest book shows why. *Science: Good, Bad and Bogus* is a selection of Gardner's writing over the past thirty years, and it reveals his passion for the truth.

Despite the three-decade span the articles exhibit a consistency in approach and attitude. His criticism of pseudoscience is exacting and at times devastating, all the more so because of his willingness to give his opponent the benefit of the doubt.

Michael Dennett is a writer and critic of fringe-science claims. He wrote "The Bermuda Triangle, 1981 Model," for our Fall 1981 issue.

Science: Good, Bad and Bogus is an attempt to portray the role of pseudoscience in our lives. As a major force in society its effects can sometimes be far from harmless. Writes Gardner in his introduction: "There are people who have died needlessly as a result of reading persuasive books recommending dangerous diets and fake medical cures. The idiocies of Hitler were strengthened in the minds of the German people by the crackpot theories of anthropology. In recent years many children have become seriously disturbed by reading books and seeing movies about haunted houses and demon possession." With this said the author wades into the enemy.

The book covers everything from dermo-optical perception (the ability to read with the fingers) to talking apes. Of particular importance is Gardner's treatment of parapsychology, which comprises a significant percentage of the book. The Israeli psychic Uri Geller and Ingo Swann fall under the author's persuasive criticism. Parapsychologists Targ, Puthoff, and Rhine are criticized for their Catch-22 approach to failure in psi experiments: "In other sciences failure . . . to replicate an experiment is counted as disconfirming evidence. Because psi powers are said to be adversely influenced by doubt [Catch-22], however, parapsychologists are not impressed by replication failures unless they are obtained by sheep [nonskeptics]." When such replication does not occur even when carried out with believers, as happened to Targ, Gardner explains that Targ "turned to Catch-23. Catch-23 asserts that psi powers are negatively influenced by complexity." Parapsychology, it seems, has much in common with astrology, another alleged science with a proliferation of loopholes. Not everything said in the book about ESP is negative. Gardner writes that he regards "psi phenomena as possible but 'very doubtful,'" and that "I find myself in full agreement with J. B. Rhine and other parapsychologists who regard the lack of a physical theory as no obstacle whatever to the acceptance of psi." These statements by the author clearly show that he is open-minded, contrary to the claims of proponents of ESP.

Gardner explains in his introduction: "I do not believe that books on worthless science, promoted into best-sellers by cynical publishers, do much damage to society except in areas like medicine, health, and anthropology." Such a mild statement about pseudoscience by so strong a critic of the paranormal reflects Gardner's inclinations to give almost anyone the benefit of the doubt.

The author has no doubt, however, about the uselessness to society of medical charlatans. In his review of John G. Fuller's book, *Arigo: Surgeon of the Rusty Knife,* Gardner swings his "bat" with extra measure. Arigo, who lived in Brazil and whose education stopped at the third grade, practiced illegal medicine for twenty years. He used no anesthetics and no antiseptics. Fuller tells story after story of how this psychic healer cured lung and uterine cancer, performed delicate eye operations with a penknife, and on occasion even operated with the "dull side of a knife or even just a finger."

"As a documented study, Fuller's book is worthless," writes Gardner. He adds that, although "Arigo was violently opposed by the Catholic church and by Brazilian doctors, not a paragraph in the book presents the views of any informed person who did not buy the Arigo mystique." Gardner reveals that Arigo was twice sentenced to prison and that his brother ran a local pharmacy in which thousands of patients purchased drugs prescribed by Arigo. Gardner finds himself led to "a serious moral question. Fuller's book is just persuasive enough to convince some fuzzy-minded readers, with curable ailments, to stop seeing their physicians and fly

to Brazil or to the Philippines to be mangled by psychic quacks."

Other topics covered in *Science: Good, Bad, and Bogus* include a short exposé of Ruth Carter Stapleton, sister of Jimmy Carter, and an article about the original biorhythm quack, Wilhelm Fliess. (Fliess, it turns out, was an intimate friend of Sigmund Freud.) Perhaps the most interesting part of the book is an essay about Conan Doyle in which Gardner suggests that Sherlock Holmes was the creation of someone other than Doyle. This is certainly a provocative article of great interest. At first glance, an article about who created Sherlock Holmes would seem out of place in a book on pseudoscience. It is not; for Doyle was, in his day, one of the strongest proponents of numerous anomalous phenomena, including the existence of fairies.

Science: Good, Bad, and Bogus is a compilation of previously written articles and as a result is sometimes repetitive and suffers in continuity. Many of the adverse effects of such a collection are mitigated by introductory sections and postscripts for nearly every article. Of particular significance is Gardner's inclusion of adverse letters from authors and pseudoscientists he has criticized. Many of these are humorous, some ludicrous. John G. Fuller, unhappy about Gardner's statements already cited, writes that he found the review "an hysterical diatribe (certainly not a critique) by a person who is so afraid to face facts and history that he descends to unprecedented levels of calumny." A Velikovsky fan replies: "For over a quarter of a century, Martin Gardner has been taking cheap shots, grossly ill-informed and maliciously irresponsible, at the work of Dr. Immanuel Velikovsky." Allen Spraggett, offended at Gardner's review of his book on Arthur Ford, replied to the *New York Review of Books* by saying "Mr. Gardner is an expert in the art of the smear." Clearly, Gardner has irritated many of the proponents of the paranormal. The inclusion of pieces critical of his work is commendable; more important, the angry letters of protest at times reveal more about the proponents than anything Gardner has labored to write.

I do not think the title is very descriptive, but overall Gardner has produced an interesting and extensive review of the world of the paranormal. It would be a valuable contribution to any skeptic's bookshelf. Avon has just published this book as a mass-market paperback, but be forewarned that the print is small and the index is not included. ●

UFOs on NOVA

"The Case of the UFOs." NOVA, Telecast on Public Broadcasting Stations October 12, 1982. A WGBH/BBC-TV co-production, John Groom, producer.

Reviewed by Ronald D. Story

Given the kind of sensationalist journalism that has become a standard product of

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Ronald Story is author/editor of two books on UFOs and two books on ancient-astronaut claims.

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the nationally distributed tabloids and commercial TV networks, it is refreshing to see a bit of honesty and responsible reporting for a change. It is probably a safe bet that the recent NOVA program on UFOs will not be remembered as great entertainment, but its educational value rates high on my list.

The emphasis was clearly on such misidentifications and hoaxes as the alleged astronaut sightings, the Warminster UFO-photo hoax, the Travis Walton abduction case, the Val Johnson car-stopping incident, the New Zealand UFO movies, and others. Most of what the show included was well done and provided useful information, but I was disappointed with some of the editorial decisions that were made.

Realizing the obvious difficulties involved in editing many hours of film down to only one, the final selection of topics and people to be included did not seem to me to be the very best. The segments with Allan Hendry, Jim Oberg, and Phil Klass were interesting and informative. But astronomer J. Allen Hynek, who was interviewed on camera, at length, was left on the cutting room floor. And despite my own inclination toward skepticism in UFO matters, I would like to have seen more of a forum, with representatives on both pro and con sides having their own say. More often than not, the alleged UFO witnesses (in short supply on the program but abundant in reality) and leading proponents (such as J. Allen Hynek and Stanton Friedman) can be easily countered by skeptics (such as Oberg and Klass), but to eliminate them completely from view is neither objective nor scientific.

This reviewer also favors more emphasis on the psychological and sociological dimensions of the UFO problem, as opposed to watching more talking-head documentaries of old Air Force films and Project Blue Book findings that almost everyone agrees were blatantly superficial.

The fact is that the entire case for UFOs rests ultimately on human testimony, and so that is where I think the focus of UFOlogy—and of new documentaries on the subject—should be today. Let's concentrate on the psychological and cultural factors that produce the individual *interpretations* we receive as UFO reports. Here, in my opinion, is where the scientific paydirt lies—if anywhere.

For example, I think that some fascinating material could have been presented on behalf of Alvin Lawson and William McCall in connection with their imaginary-abductee experiments. Instead, an inordinate amount of precious time was wasted on Michael Persinger's highly dubious "theory" of some novel type of earthquake light, or piezoelectric-tectonogenic-electromagnetic plasma, that induces only UFO-abduction type hallucinations in the hippocampus of the brain. Then, after this "explanation" was given to account for the well-known Travis Walton case, it was revealed that Walton had flunked a lie-detector test, indicating that the entire incident was a hoax! Well, which is it? You can't have it both ways! In any event, impressive technical jargon notwithstanding—and even though much of the data going into the theory was obtained from such highly scientific sources as the books of Charles Fort and the pages of *Fate* magazine—Persinger's so-called "theory" has so little merit in explaining the UFO phenomenon as to enter the category of the ridiculous. And unfortunately, this category is overflowing at the moment.

The NOVA documentary was at its best in its coverage of the alleged UFO sightings by astronauts and the infamous New Zealand episode. These were neatly debunked and the record set straight.

Whatever its shortcomings, this film has presented a reasonably accurate, though necessarily abbreviated, view of the UFO phenomenon and related problems. It certainly gave us more truths than falsehoods in a subject area that is particularly prone to the latter. That alone makes the film a valuable contribution to the subject. •

Half-Baked Star Lore

The Night Sky. By Richard Grossinger. Sierra Club Books, San Francisco, 1981. 481 pp. $17.95.

Reviewed by Jim Swanson

While out stargazing one night (some say in the company of a serving wench), the Milesian astronomer Thales tripped into a puddle (some say off a cliff). The elders scoffed: Had he been looking down, Thales could have seen the stars reflected in the water; but, looking aloft, he could not see the water in the stars. In *The Night Sky*, Richard Grossinger, a long-winded apologist for astrology, glimpses the water in the stars when he admits that "the millennial sciences have no allegiance to transient forms." This does not shed enough light to prevent his downfall. As he lumbers toward the Aquarian Age, Grossinger slips and stumbles down nebulous chains of reasoning into vacuous conclusions.

Grossinger believes that "native man" was in touch with the heavens but that we moderns have lost our grasp. He diagnoses in today's society a mental disorder "so vast and general that we do not notice it anymore." Scientific investigation has "reduced the assessment of living and intelligent beings." "Formal uncertainty theory" is reflected in the "uncertainty that has become a way of life." Currency inflation is connected with the expanding universe. "Radio telescopes showed us that this was a universe in trouble." Black holes, pulsars, and quasars may seem safely distant today, but Grossinger suffers in the knowledge that these violent renegades must be "hot upon us, because the expanding universe cannot hold them at bay forever." Our only hope is in the realization that the "stars are no more inanimate and unwilled than we are" and that we are "in some primary sense, replicas of the heavens at the moment of our birth."

Two confessions rent the fabric of this melodramatic weaving of moralistic conceits and shoddy astronomy. Grossinger admits to being brainwashed by the happy endings to "Star Trek" television episodes. And he declares that, "while doing deep-breath Lomi work with Polly Gamble, I began *The Night Sky* in my mind. Or it began there despite her directive to empty my mind of all thoughts"—a directive only half-obeyed, in keeping with his other half-baked declarations.

An unusually curious report in this uncritical mass of forgettable lore concerns the famous cattle mutilations "which have been done with an extremely sharp surgical instrument by intruders who arrived in some sort of flying vehicle."

Jim Swanson is a newspaper editor in McBride, British Columbia.

Grossinger concurs with the "French scientist" Jacques Vallee, who has pointed out that the mutilations "cast a pall over the whole UFO phenomena." "What else," wonders Grossinger, "can we expect from creatures who cross millions of miles or light-years to come to our planet, and disfigure harmless and helpless domestic animals?" What else, indeed? Perhaps these wicked creatures will broadcast psychotronic vibrations that will cause hapless authors to produce books like *The Night Sky*. Heaven help us. •

Some Recent Books

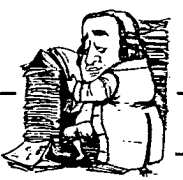
Listing here does not preclude more detailed review in a future issue.

Collins, H. M., and T. J. Pinch. *Frames of Meaning: The Social Construction of Extraordinary Science.* Routledge and Kegan Paul, Ltd., Boston and London, 1982, 210 pp. $27.50. Two English sociologists of science examine the "rationality debate." The first section deals with the theoretical framework of parapsychology's relationship with science. The middle section is devoted to the 1975 empirical studies by Brian Pamplin and Collins at the University of Bath of children who claimed paranormal abilities of metal bending (they were observed to cheat). The final section deals with unreported details and sociological aspects of those experiments and with issues of "experiment and paradigm."

Oberg, James E. *UFOs and Outer Space Mysteries: A Sympathetic Skeptic's Report.* Donning Company, Norfolk, Va., 1982, 192 pp., $6.95 paper. A collection of the author's writings on UFOs, close-encounter tales, moon-mystery myths, the Sirius mystery, and other related themes. The book's Introduction is an expanded version of his award-winning Cutty Sark essay; the Afterword is his assessment of UFO research at the Smithsonian UFO symposium. Oberg always brings a welcome freshness and critical but open-minded skepticism to his work.

—*K.F.*

Articles of Note



Anderson, Alan. "How the Mind Heals." *Psychology Today*, December 1982, pp. 51-56. Report on research in the new field of psychoneuroimmunology, which is producing data that seem to prove a direct physical connection between the brain and the immune system. Excellent nonmystical

examination of the immunological links between mind and disease.

Beck, Stanley D. "Natural Science and Creationist Theology." *BioScience* 32:738-742, October 1982. A biologist who is a Christian discusses why so-called scientific creationism is not scientific and why its biblical basis is "unacceptable theologically." See also the editorial "Why We Should Not Teach Creationism in the Schools," by C. F. Herreid II, in the same issue.

Blackmore, Susan. "Parapsychology—With or Without the OBE?" *Parapsychology Review* 13(6):1-7, November-December, 1982. Paper argues that the so-called out-of-body experience (OBE) may be a purely psychological phenomenon, requiring no paranormal infuences for its explanation. Author then speculates on parapsychologists' "stark choice": Will they drop their interest (leaving psychological research on all such interesting matters to others and making parapsychologists yet more isolated) or continue to embrace such subjects, even if no paranormal explanations need be invoked? This is the paper Blackmore delivered at the parapsychology meeting in London in August 1982.

Broad, William J., and Nicholas Wade. "Science's Faulty Fraud Detectors." *Psychology Today*, November 1982, pp. 51-57. Examination of issues of scientific fraud finds that science's three principal mechanisms of self-policing—peer review, the referee system, and replication—are faulty. In particular, scientists rarely bother to replicate one another's experiments, and those replication attempts that have negative results are frequently discounted. Since access to the raw data of an experiment is seldom available to outside scientists, Broad and Wade suggest that insiders in the same laboratory actually provide the first and most important check on a researcher's work. From the authors' new book *Betrayers of the Truth* (Simon and Schuster).

Brown, George W. "A Loss of Nerve." *Developmental and Behavioral Pediatrics* 3(2): 88-95. A physician laments the "loss of nerve" in confronting "foolish claims, fads, and pseudoscience" in the biomedical fields and gives many examples. He worries over a general "retreat from knowledge" and finds that the conviction and enthusiasm for some therapy fads and folklore seem almost to "grow in inverse relationship to the factual support for the proposed idea."

Campbell, David E. "Lunar-Lunacy Research: When Enough Is Enough." *Environment and Behavior*. 14(4): 418-24. A critical response by a psychologist to an article in the same issue by Salvator J. Garzino. Garzino's article defends empirical research allegedly linking lunar phase and human behavior; Campbell criticizes Garzino's defense.

Cohen, David. "Paradigms of the Paranormal." *Nature* 299:505, October 7, 1982. Review of a new, "supernaturally expensive" ($125/year) British quarterly journal of paranormal research, *Psychoenergetics*, finds its contributors "curiously unaware" of the many problems and criticisms of the field's methodology.

Cowen, Robert. "Kooks, Comets, and Creationism." *Technology Review*, November-December 1982, p. 6 ff. Examination of the comet-catastrophe theories of Clube and Napier and their Velikovskian overtones and the even more "far out" comet-seeding theories of Hoyle and Wickramasinghe and their religious overtones. The latter pair's is "wild stuff" scientifically, but

Cowen suggests the scientific community's "polite skepticism" is partly because Hoyle and Wickramasinghe, in contrast to Velikovsky, are "heretics within the fold."

"Creationist Discussion Continued." *Physics Today*, October 1982, pp. 11+. Lengthy section of letters responding to letters in the June issue that responded to a report on creationism in the February *Physics Today*.

Gardner, Martin. "Nuts about PK." *Nature*, 300: 119-120. Review of the book *Psychokinesis* by British writer John L. Randall proposes that most professional parapsychologists will be embarrassed by it. Randall "buys almost everything on the psi scene no matter how flimsy the evidence." Gardner also finds the book hopelessly out of date. For example, it makes much of John Taylor's 1973 endorsement of Uri Geller without noting Taylor's 1978 reversal and denouncement of psychic metal-bending as fraud.

Herbert, Wray. "An Epidemic in the Works." *Science News* 12:188-90, September 18, 1982. Interesting article on recent cases of mass hysteria, "psychological epidemics."

Jonas, Gerald. "UFOs: We May Need Them More Than They Need Us." *Dial* (Public Broadcasting Communications, Inc.), October 1982, pp. 10-12. Perceptive essay by a *New Yorker* staff writer on the psychology of UFO belief.

Krupp, Edwin. "Dragons of Chaos." *New Scientist*, November 4, 1982, p. 309. Review of *The Cosmic Serpent* by Clube and Napier, which posits that many myths and events in historical times are explained by the impact on earth of spent comets during the past 5,000 years. Half the book, on astronomy, Krupp finds "intriguing"; the second half, "the slings and arrows" part, "silly." Here their "logic is as faulty as their facts."

Numbers, Ronald L. "Creationism in 20th Century America." *Science* 218:538-44, November 5, 1982. Review article by science historian on the historical development of "scientific creationism."

"On Astrology." *Psychology Today*, November 1982, p. 6. A brief exchange of letters between Michel Gauquelin and George Abell about Abell's article "The Mars Effect" in the June *PT*.

Patterson, John W. "An Engineer Looks at the Creationist Movement." *Proceedings of the Iowa Academy of Sciences* 89(2):55-58, 1982. Observations by a concerned engineering professor who notes that "engineers are perhaps the most prominent leaders of the creationist movement," a fact he characterizes as a "travesty." Patterson concludes, among other things, that knowledgeable scientists and educators have a responsibility to expose the "scientific incompetence" in the creationist movement and that "public schools that willfully adopt the educational materials produced by such incompetents deserve to be disaccredited."

Rosenbaum, Ron. "Turn On, Tune In, Drop Dead." *Harper's*, July 1982, pp. 32-42. An acerbic look at the "apostles of drop-dead chic," Kubler-Ross's death-and-dying movement.

Story, Ron. "UFOs on the Brain." *Frontiers of Science*, July-August 1982. Essay suggesting that the UFO anomaly is at bottom a psychological one, "a powerful mythology."

"That *Is* Incredible." *The ISC Newsletter* (International Society of Cryptozoology), vol. 1, no. 2 (1982). Editor J. Richard Greenwell reports that a film shown on

the "That's Incredible" TV program in December 1981 of a "dinosaur" rising out of a river in Africa was actually a film of a carved wooden model made to show what the hypothetical creature Mokele-Mbembe might look like if in fact it existed. Told of this, the producers replied that the program is not a scientific forum but an entertainment show.

Truzzi, Marcello. "J. B. Rhine and Pseudoscience: Some Zetetic Reflections on Parapsychology." In K. Ramkrishna Rao, ed., *J. B. Rhine: On the Frontiers of Science*. McFarland, Jefferson, N.C., 1982. Pp. 177-191. Review of the three major forms of criticisms of parapsychology—empirical, conceptual, and methodological. Criticizes psi researchers for not insisting on the presence of magicians during experiments and calls for greater integration of parapsychologists' concerns with those of general psychology.

—*Kendrick Frazier*

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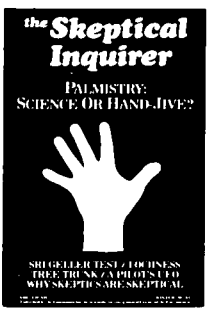

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Follow-up

The Abell-Kurtz-Zelen 'Mars Effect' Experiments: A Reappraisal

From 1976 through 1980, UCLA astronomer George O. Abell, SUNY-Buffalo, philosopher Paul Kurtz, and Harvard statistician Marvin Zelen were involved in two experiments intended to test the validity of certain aspects of French psychologist Michel Gauquelin's claims for a correlation between planetary positions in the sky and the birth times of notable professionals. Some of our public statements about the tests, our procedures, and our reports of the results have been criticized. In our judgment some of these criticisms are valid and should be acknowledged even though they do not negate the general results. The purpose of this commentary is to set the record straight and clarify what we regard to be the status of the Gauquelin planetary-effects study. We wish to make note of some of the errors in our earlier analyses in order to contribute to ongoing scientific inquiry, which we believe should be a corrective process.

The Mars Effect

For the purpose of his investigation, Gauquelin divides the sky into twelve sectors, fixed with respect to the horizon; six of these sectors lie above the horizon and six below. As the earth rotates, objects in the sky, including each of the planets, must pass through each of these twelve sectors. Gauquelin regards two of the sectors, the one lying just above the eastern horizon (which contains objects that have risen within, roughly, the past two hours) and the one just west of the local meridian (which contains objects that have passed from the east to the west half of the sky during, roughly, the past two hours) as key sectors. He has found that at the birth times of certain notable professionals certain planets appear in key sectors either more often or less often than one would expect by chance. One of his claims, which we have dubbed the "Mars effect," is that Mars is in key sectors more often than would be expected at the times of birth of sports champions. Gauquelin's original sample contained 1,553 champions. In the 1960s he persuaded a committee of skeptical Belgian scientists, the Comité Para, to examine the effect. In consultation with Gauquelin the Comité Para agreed on directories of famous sports champions. With the aid of L. de Marre, Gauquelin gathered for the Comité Para the data on birth times for 535 champions in those catalogues. In both the new sample and the original one Mars appeared in key sectors at the time of birth of 22

percent of the champions. According to Gauquelin's calculation, the expected percentage should be about 17 percent if there is no Mars effect.

The Comité Para, however, did not endorse the effect as substantiated because it did not believe that Gauquelin had properly allowed for all demographic effects in the calculation of the expected fraction of champions that would be born with Mars in key sectors if the Mars effect were absent. The complication occurs because people are not born with equal frequency during all hours of the day; the birth rate peaks in the early morning and is lowest in the late afternoon. This nycthemeral effect must be combined with the astronomical circumstance that, as seen from Earth, Mars is more often near the sun in the sky than far from it. The astronomical factor can be allowed for rigorously, but the calculation of the expected distribution of Mars sectors still requires some assumption about the nycthemeral curve (the distribution of births during the day in the general population) as it applies to the times and places of the births of the champions. Gauquelin had found that the nycthemeral curve for the combined sample of champions (all 2,088) closely resembles that for the general population. One of us (Abell) has confirmed that this is true if the birth data published by Gauquelin are correct. It thus seemed unlikely that, for the general population of people born at the times and places of the champions, the fraction of births with Mars in key sectors would be much different from the calculated 17 percent. Others, for example, Dennis Rawlins, had arrived at similar conclusions. Nevertheless, the question of the theoretical distribution of births with Mars in key sectors had not been resolved rigorously.

The Zelen Test

A way around the problem was proposed by Marvin Zelen in 1976. The Zelen test is very simple: (1) select at random a subset of champions from the sample of 2,088;* (2) obtain from birth registries the birth data for everyone else born on the same dates and at the same places as the champions; (3) compare the percentage of births with Mars in key sectors among the champions with that among the nonchampions. It was Zelen's view that by this test we could bypass the question of the theoretical frequency. It seemed like a sensible test to undertake and the Gauquelins volunteered to do so.

So great was our respect for the Zelen test that two of us published careless statements about its power. In the *Humanist* (Sept./Oct. 1976) Abell described it as "a very definitive test." In the *Humanist* (Jan./Feb. 1976) Zelen wrote, "We now have an objective way for unambiguous corroboration or disconfirmation." The Zelen test is indeed a definitive way of avoiding any uncertainty over the method of calculation of the theoretical percentage of births with Mars in key sectors, but of course could not be immune to sample irregularities, such as biased or fudged data. We ought not to have made such sweeping statements and regret our carelessness.

Gauquelin carried out the Zelen test by selecting a subsample of 303 champions from the 2,088, and he obtained birth data on 16,756 other people born in the same towns and within three days of the same dates as the champions. To obtain a sufficient sample, Gauquelin found it necessary to deviate from the agreed-upon procedure for the sample selection and chose only champions born in

*Originally Zelen suggested that a sample of 100 to 200 would be sufficient; but, as Gauquelin pointed out, a larger sample is required to detect an effect as subtle as that of the Mars effect.

the *chef-lieux departements* in France and Belgium. This means that, in pooling the results, large cities had contributions in the sample not related to their proportions in the population. If the Mars effect is unrelated to whether an individual is born in a large town or a small town, then the disproportion from one place will have no effect. Gauquelin's justification, along with the results of the test, were published in the *Humanist*, Nov./Dec. 1977, pp. 30-35. In agreement with Gauquelin's prediction, 22 percent of the 303 champions had been born with Mars in key sectors, while 16.4 percent of the control group of 16,756 were born with Mars in key sectors.

We have ascertained that the birth data on the 303 champions selected by Gauquelin in carrying out the Zelen test are the same as those published in his 1970 document giving data for the full sample of 2,088 champions. Moreover, Harvard astronomer O. Gingerich arranged to have the calculations of the Mars sectors spot-checked for about 2,000 of the nonchampions, and no discrepancy was found. On the other hand, we did *not* check thoroughly the original selection of 2,088 champions, nor whether all of the reported birth data are in agreement with those in the birth registries. One of us (Kurtz) did spot-check the data Gauquelin presented for the champions; and, although he found numerous descrepancies, Gauquelin was able to explain most, if not all, of them. Kurtz found that Gauquelin's files were meticulous and well organized, and on June 24, 1977, Gauquelin and Kurtz signed a statement to the effect that the files had been examined and found in good order. In fact, it was not within our resources to carry out the kind of check necessary to insure against any possible bias, and the signing of the above-mentioned statement was, in retrospect, open to misinterpretation; we regret the error in doing so. The point is that we did not and could not check on the accuracy of all of Gauquelin's data, much of which was unavailable for scrutiny. In no case does any of this imply that any of us has "carefully controlled the objectivity of the selection," as Gauquelin has claimed.

Following Gauquelin's report on the Zelen test, we (Zelen, Kurtz, and Abell) published a critique in the same issue of the *Humanist* (Nov./Dec. 1977, pp. 36-39). In our judgment, there are some valid criticisms of our statement.

1. We neglected to mention specifically that the Mars effect (at least for this sample) was not due to some combination of astronomical and demographic factors, as suggested by the Comité Para. (The Gauquelins did make this point in their report preceding ours.) In other words, Gauquelin was correct in his prediction that 17 percent of ordinary people would be born with Mars in key sectors. Our omission was an unintentional oversight that resulted from the principal author (Zelen) being primarily concerned with the significance of the sample at hand (303 champions) and not with the previous study or the predictions based on it. Kurtz was concerned with the administration of the project, and Abell, who should have caught and avoided the omission was heavily involved with other projects and neglected to examine the manuscript with enough care.

2. Because only 9 of the 303 champions were female, but roughly half of the control sample of 16,756 were female, we eliminated the 9 female champions from our discussion; and, in the spirit of "comparing like with like" compared the remaining 294 male champions with the 8,613 male nonchampions. We believe that such a comparison is valid. Our critics have stated that the females should not have been dropped from the analysis because: (*a*) there was no original agreement to consider only male champions; (*b*) three of the female champions had Mars in key

sectors, and their elimination reduced the statistical significance of the difference between champions and nonchampions from one that would occur by chance only 2.5 percent of the time to one that would occur by chance 4 percent of the time. We regret not having discussed the entire sample before comparing male athletes with male nonathletes.

3. Because of the champion sample size (303), the significance, for that sample, of a Mars effect in which only 22 percent, as opposed to 17 percent, of the athletes have Mars in key sectors is necessarily not great. We pointed out that removal of even a single champion with Mars in a key sector could change the significance level of the result to one that would not generally be considered significant. This discussion was presented to show the sensitivity of the analyses to a single observation. This sensitivity analysis was objected to by some readers as representing a biased analysis of the data. In retrospect, this should not have been included in the original paper because one can always reduce the significance of a distribution by removing elements from the sample. Furthermore, we had already removed 9 females, of which 3 had Mars in key sectors. Moreover, our analysis did not stress the point that the main result of the Zelen test was to remove a principal objection to the manner in which the Gauquelins had calculated their "theoretical" Mars-sector distribution; consequently there remains no reason to question (if the data are representative) the significance of the difference between the observed 22 percent and the predicted 17 percent for the entire sample of 2,088 athletes. This presentation did not result from any attempt to obscure the Zelen test results; it is nevertheless regrettable that some critics so interpreted it.

4. We have been criticized further for dividing the sample of 303 into separate, smaller samples of champions from Belgium (where the Mars effect was completely absent), France excluding Paris (where it was marginally significant), and Paris (where it was highly significant). Our analysis discussed the entire male sample and partitioned it according to geographical region. The reason for the partitioning was to examine geographical variations, since large-population areas were given disproportionate weight in the combined analysis. It was not our purpose to divide the sample to lower the significance of the results. This sort of exploratory data analysis is common in analysis of complicated data sets, but only for generating hypotheses to be tested, not necessarily for generating conclusions. We should add that there is no clear evidence that the Gauquelins intentionally biased the sample. We regret that some may have interpreted our comments otherwise.

In summary, the purpose of the Zelen test was to avoid any question over the proper way to calculate the theoretically expected distribution of Mars among the sky sectors at the times of birth of those belonging to the general population. It is our judgment that in this instance, at least, the procedure used by Gauquelin appears to have been vindicated. To say this, however, in no case verifies that the Mars effect is real, for we had no way to assure that the sample selected by Gauquelin was unbiased. It was our opinion that the best procedure available to us was to attempt to replicate the effect with a completely independent sample of sports champions born in the United States.

The American Champion Study

In 1977 and 1978 we obtained from state registries the birth data for 408 American sports-champions, selected from what we regarded to be standard directories of famous sports-champions. San Diego astronomer Dennis Rawlins calculated the

Mars sectors for these athletes and carried out his own analysis of the results. Mars appeared in key sectors at the birth times of 13.5 percent, even less than (but not significantly less than) the expected 17 percent. This study is reported in our article and in one by Rawlins, both in the SKEPTICAL INQUIRER, Winter 1979-80, pp. 19-31.

Gauquelin, however, maintains that our sample of great American athletes is contaminated with athletes of lesser stature who do not show the Mars effect. Gauquelin has selected from our list a subsample of 88 U.S. sportsmen who he regards as truly outstanding. In a rejoinder to both our article and Rawlins's article in the same issue of the SKEPTICAL INQUIRER (pp. 31-43), Gauquelin and his wife, Francoise, point out that 21.6 percent of these 88 athletes do have Mars in key sectors, and argue that even the U.S. sample shows the Mars effect. In a final response in the same issue of the journal (pp. 44-59), we pointed out that the size of his subsample is far too small for the results to be meaningful and, moreover, that it is improper in statistical studies to select a sample *post hoc* from data for which the results are already known. We then proceeded to demonstrate how other *post hoc* selections that would seem equally justified could give the opposite result.

We now believe that there are some valid criticisms of our discussion of the U.S. test.

1. First, we ought not to have followed the Gauquelins in suit by playing the game of *post hoc* selection from the data. Our purpose was to demonstrate that one can often obtain any result one wants by such a procedure, but we have likely been misunderstood. In retrospect it would have been better had we merely stated that such *post hoc* selection is inappropriate.

2. The criticism has also been made that in our introductory article, in which we reviewed briefly the earlier work, including the Zelen test, we reiterated the errors mentioned in paragraphs (1), (2), and (4), above. We regret repeating our earlier oversights.

3. Most important, much of the debate over the outcome of the study of American athletes could have been avoided had we obtained in advance a clear understanding with the Gauquelins on exactly what they were predicting and what directories of famous sports champions would be satisfactory according to their hypotheses. Before conducting our test, we did discuss with Gauquelin how an American test might be carried out and what directories might possibly be used, but there were no written agreements. Moreover, it would have been far better if we had agreed upon neutral judges to supervise the entire study, so that no party could be accused of bias.

Although we wish we had proceeded according to point 3, above, we wish to emphasize that we were under no scientific or moral obligation to do so. We were aware of the surprising "Mars effect" association Gauquelin had reported for champion athletes and were attempting an independent replication. This is standard procedure in scientific inquiry. Neither Gauquelin's permission nor his approval was required. However, his agreement, with firm controls, might have avoided the later bickering over the relevance of our sample.

In spite of these shortcomings, we do believe that our U.S. study was a valid one and that it gave results highly inconsistent with a Mars effect for athletes. For example, even if we were to allow Gauquelin the point that the subset of 88 of our sample of 408 sportsmen comprises truly great athletes and shows a Mars effect (albeit not with statistical significance, possibly due to the small sample size), then the remaining 320 athletes, who were all successful enough to make highly selective

directories in a highly competitive field and who should certainly show some of the Mars temperament (if real), show a very negative Mars effect! Are we to believe that the surprising Mars effect present for the very top athletes is not only absent but actually negative (compared to us ordinary folk) in the near-great competitors?

Concluding Comments

While we are more than willing to acknowledge our mistakes and shortcomings as we become aware of them, we do not want to leave the impression that the Mars effect is in any sense confirmed. We believe that the results of the Zelen test suggest that Gauquelin adequately allowed for demographic and astronomical factors in predicting the expected distribution of Mars sectors for birth times in the general population. On the other hand, such a highly unexpected phenomenon as the Mars effect must, in our judgment, be confirmed independently and under strict controls to rule out data bias or manipulation. This has not been done either in the Gauquelins' studies or in the negative test on U.S. athletes.

Understand, there is no question about some cosmic influences. Solar radiation on the earth, modulated seasonally, is responsible for most available energy on earth, and of course for life itself. The moon not only reflects sunlight to us but is the principal agent in raising tides. These and many other natural effects are thoroughly understood and are explicable in terms of known laws of nature.

Gauquelin's planetary effects, on the other hand, are not explicable in terms of known laws. Neither the Gauquelins nor anyone else has advanced a satisfactory hypothesis for a causal mechanism that could explain a correlation. The Mars effect, to be real, would require new physics beyond anything that science can at present understand. To be sure, a lack of an explanation for a phenomenon is not in itself sufficient reason to reject it; but it is reason for checking it out with great care. Thousands of bizarre results are reported each year that fail to stand up under careful scrutiny.

Our skepticism for the Mars effect (and the other Gauquelin correlations) is not based on a distaste for it. On the contrary, it would be a very interesting challenge to find positive evidence that such a surprising effect was real.

In summary, we regard it as improbable that the Gauquelin planetary effects are real. Yet, because they suggest hitherto unknown cosmic influences, they have great appeal to astrologers, and the subject has thus received worldwide attention. We regret that at the outset we had not the foresight to exercise a great deal more care in our experiments and in reporting them. Had we done so, we might have been able to reach conclusions more convincing to others. On the other hand, it is doubtful if anything we could have done would have settled the matter. The final verdict will require time and new attempts at replication. We urge future investigators to proceed with the utmost care. Until there are further independent replications, we believe that judgment should be suspended as to whether the "Mars effect" is genuine.

George O. Abell
Professor of Astronomy
UCLA

Paul Kurtz
Professor of Philosophy
SUNY-Buffalo

Marvin Zelen Chairman,
Dept. of Biostatistics
Sidney Farber Cancer Inst.
Harvard University

Project Identification: Author Replies

Robert Sheaffer's review of my book *Project Identification: The First Scientific Field Study of UFO Phenomena* (Prentice-Hall, 1981) in the Spring 1982 issue is loaded with mistakes, distortions, omissions, insults, and statements taken out of context. Worst of all, his woeful attempts to show technical inconsistencies are incorrect.

Scheaffer takes up the cudgel by claiming errors of observation during our very first night of viewing: "One of the first UFO sightings was dismissed by a skeptic who also saw it as an automobile far off *on* the horizon." (Italics added.) The *skeptic*, an astronomy instructor in my department, did *not* observe through available optical instruments; *I was the one* who observed through the Questar telescope. On each of five short appearances, a stationary ball of light was at different distances and angular altitudes, showing from five to ten degrees of sky background *under* it. My colleague's suggestion that we were observing car headlamps was absurd. Cars do not hover in the sky! Even our wives—each had binoculars—chided my obstinate colleague for his wild assertion.

To further refute his claim, I made a simplified calculation of resolution showing that I could have distinguished two headlamps up to about forty miles distant. Sheaffer assumes: "But he *utterly* ignores the effect of atmospheric turbulence and scattering, which distorts distant images and destroys a telescope's power of resolution." (Italics added.) Atmospheric conditions were not a serious factor because, on 80X, the telescopic image had a well-defined edge, which suggests to any knowledgeable scientist that the light was much closer than forty miles. Visual clues on the night of the sighting and a daylight search by air for a road on a mountaintop confirmed my belief that the light was only about twenty miles away at its *maximum* excursion. Further, he ignores my technical discussion of resolution and of the scintillating-star effect, for which I discovered that hovering "vehicles" flash controlled amounts of red, white, and green light in predetermined time increments to produce a scintillating pseudostar to match the color and brightness of a nearby prominent star.

Another night, simultaneous time exposures of a moving light were made using lenses of 50-mm and 800-mm focal length. The exposure using the 800-mm lens showed a trail of light with familiar camera vibration, but the exposure using the 50-mm lens showed hundreds of small structured lines and dots. Because the 50-mm lens provided more detail, Sheaffer *rejected the data!* He continues: "This violates *every* law of optics and should be an immediate tip-off to any scientist that the supposed fine movements of the object were due entirely to camera motion." (Italics added.) Every person, scientist or otherwise, who saw my photographs immediately jumped to the same erroneous conclusion that the intricate pattern on film was caused simply by camera vibration—until I began to query them. Of course Sheaffer conveniently ignores my more sophisticated yet conventional explanations. Also, he seemed to be unaware that the speed of a photographic system is modified when the object is moving, a point thoroughly discussed in the Appendix.

From here on the review deteriorates. He objects to my statement that National Guard helicopter pilots searched for UFOs. One local pilot admitted to nightly searches. Another told me that he flew fifty miles on a "training flight" to be

in an area where UFOs were seen frequently. Does Sheaffer believe that I made up these stories—that I would jeopardize project data by lying? He should hear the stories from the military that I left out of the book.

Sheaffer states: "Rutledge is *utterly* convinced that the group 'interacted with the phenomenon under study,' and claims that it had 'an intelligence equal to or greater than that of man.'" (Italics added.) Throughout the book, I carefully used the phrase *"apparently* reacted," the adjective denying scientific claim. The "equal to or greater than" statement is a pure logic statement, its meaning obvious to any freshman mathematics or physics student.

In the chapter where I review the history of UFOs, Sheaffer chastises me for mentioning that *SAGA* magazine, quoting from CIA papers released through the Freedom of Information Act, stated that the Nazis successfully flew discs during World War II. He failed to say that several years earlier I had received a letter from a German that contained photographs and details of the *alleged* flying-saucer project.

Sheaffer belabors me for suggesting the possibility of telepathy in two of the 157 project-sightings, but ignored my more conventional explanations. He objects to my including *two* sightings by my son after he became sixteen years old. The boy, then an accomplished amateur astronomer and photographer, is now a university physics major.

Sheaffer ends his charade by stating that he is embarrassed by my book. To him, "the story of *Project Identification,* is, from start to end, a massive exercise in self-delusion." Our study is the best-documented in the history of the UFO phenomenon, an average of 3.5 persons witnessing each of the 157 sightings. So who is deluded? Is it the forty scientists, engineers, students, and lay persons who worked in the field, or is it *the* avowed skeptic turned calloused cynic?

Harley D. Rutledge, Chairman
Physics Department
Southeast Mo. State Univ.
Cape Girardeau, Mo.

Sheaffer Responds to Rutledge

It is interesting that Rutledge has waited until now to reveal that the lights explained by Milton Ueleke, an astronomy instructor who also saw them, as automobile headlights, were "five to ten degrees" above the horizon. Rutledge seems to have forgotten to mention this fact in his book, where he describes them only as "near the southern horizon." Perhaps Ueleke believes that cars *do* hover in the sky, or else he would place the objects much closer to the horizon than did Rutledge. I didn't see the objects; Ueleke did. The fifth time the UFO appeared, it was said to be much higher in the sky, so if this is correct that object clearly was no car. This was the only occasion on which it was well observed in a telescope, and for only twenty seconds. In the book, Rutledge says that Ueleke's automobile hypothesis "was a rational explanation that could be checked by experiment." Above, he calls it a "wild assertion." Which is correct?

Rutledge's technical discussion of telescope resolution would be pertinent if the observation were made on the moon. Here on earth, at the bottom of an ocean of air, a telescope virtually never achieves its full power of resolution, especially for objects seen through miles of atmosphere.

The comments above on the images produced by 50 mm and 800 mm lenses show that Rutledge confuses the *speed* of a camera system, which is a function of film speed, f/number, and the lens diameter, with the *resolution* of the system, which is principally determined by the focal length of the lens. A *faster* system does not necessarily show finer detail, only brighter images. To claim otherwise suggests a misunderstanding of optics. When properly mounted and focused, a telephoto lens will *always* resolve finer details than a standard lens in the same place at the same time. If the reverse were true, it would be even more bizarre than Rutledge's UFOs.

Rutledge is now claiming that he said the objects "apparently reacted" with the observers. The book, however, says flatly, "In this Project, we dealt with an intelligence equal to or greater than that of man. We interacted with the phenomenon under study." What happened to the "apparently"? Let me share with you his "conventional" explanation offered as an alternative to "telepathy": "Possibly the UFOs were able to drift silently near our positions at night and listen to our conversations, or detect them at a distance."

I will not even comment on his suggestion that UFOs may have really been developed as Nazi secret weapons beyond saying that even the credulous UFO organizations, whose members believe in everything from crashed saucers to UFO abductions to telepathic contact with aliens, are virtually unanimous in rejecting this wild claim.

Robert Sheaffer
San Jose, Calif.

Scientists and Borderline Science

Very few scientists actually plunge into the murky waters of testing or challenging borderline or pseudoscientific beliefs. The chance of finding out something really interesting—except about human nature—seems small, and the amount of time required seems large. I believe that scientists should spend more time in discussing these issues, but the fact that a given contention lacks vigorous scientific opposition in no way implies that scientists think it is reasonable.

—Carl Sagan in *Broca's Brain*

From Our Readers

The letters column is a forum for views on matters raised in previous issues. Letters are welcome and are more likely to be published if they are typed and double-spaced. Some may have to be edited.

Nostradamus and rhyme time

I have been subscribing to the SKEPTICAL INQUIRER for approximately one year; however, already I have found it to be a most excellent and informative journal. The issue on the shroud of Turin and the one on coincidences were excellent.

The Fall 1982 issue, dealing with prophecy and the selling of Nostradamus, was especially interesting to me since I had recently seen the television program on Nostradamus, "The Man Who Saw Tomorrow," narrated by Orson Welles, which Charles J. Cazeau mentioned in his very fine article on prophecy. In connection with the television program and the writings of Nostradamus, I should like to call your attention to the following lines of verse (century and quatrain unknown), which apparently have been overlooked by Mr. Welles and other scholars of the great seer:

A great Kane, who in the October
night many did fear,
Shall, from Pandora's chromatic box,
my quatrains divine.
Reserve judgment from this portentous
teller of visions—
For ye must believe no rhyme
before its time.

Daniel J. Macero
Professor of Chemistry
Syracuse University
Syracuse, N.Y.

Astronomy and creationism

David Morrison's article on astronomy and creationism in the Fall 1982 issue is another example of a one-sided evolutionist ploy. Morrison initially states a very interesting and worthwhile argument in regard to the apparent conflict between the short age of the creationist universe and the immense amount of time required for the observed astronomical phenomena. However, he neglects a viable solution to this problem. This solution is that (*a*) God created the universe with the *appearance* of age: the light of distant stars already reaching the earth, nebulae already giving birth to new stars, the stars already in the various positions on the Hertzsprung-Russell Diagram, etc.—all of this as if it had been happening for millions or billions of years.

This would not be a "divine deception," but a principle consistent with the Genesis account of creation. After all, Adam was created as an adult (not a baby), having the appearance of years of existence.

This position is not without verification, since individual touchstones support the idea of a short age for the earth. Some of these include:

1. The earth's atmosphere contains too little helium (as a by-product) for the uranium-lead dating method to show great age. The atmosphere also contains too many carbon-14 atoms for the carbon-14 method to do the same. Other dating methods have been shown to be invalid or inconclusive.

2. There are not enough people on earth today (as descendants) to account for man's existence for a million years. Nor are there nearly enough human fossils in the earth to account for all those generations. While this does not

necessarily prove a short age for the *earth*, it does help to suggest that conclusion.

Morrison's tirade is laced with the faulty criticism that the creationist model makes the universe "uninterpretable." That model does nothing of the sort. As explained above, redshifts can occur, stars can be born and die, galaxies and quasars can exist (all operating at peak efficiency) *within* the creationist model. However, what the creationist model does prevent is the tragically unscientific theory of evolution "interpreting" us into a pool of shallow ideas and the anomaly of a highly ordered universe without purpose or meaning.

Robert Mischak
Edwardsville, Pa.

David Morrison replies:

Robert Mischak points out correctly that all of the evidence accumulated by astronomy, geology, and biology for an ancient, evolving universe can be made consistent with a literal interpretation of the biblical account of genesis, if we are willing to assume that God simply created everything with the appearance *of age and of a long evolutionary history. But this idea, while self-consistent, gets us nowhere; it is a denial of the meaning of all that we observe and measure around us. And if we accept Mr. Mischak's principle, how do we know the universe didn't start in A.D. 100, and that Jesus was also just an idea created "in place" rather than a person who actually lived? Or we could argue just as logically that creation took place at 9:00 A.M. last Tuesday, with all our memories as well as the physical evidence of the past created to give the appearance of age.*

Mr. Mischak's creationist beliefs are simply that, beliefs, based on a concept of revealed truth. If he is comfortable with such beliefs, far be it from me or anyone to try to disillusion him. But why does he go on, in the second half of his letter, to introduce spurious scientific arguments *for a recent creation? Surely if God had wanted to create all of the stars and galaxies, rock strata and fossils, and other appearances of an ancient universe, He would not have neglected to adjust the earth's radioactive clocks properly! How can the creationists reject 99 percent of the evidence for age and evolution using the argument given by Mr. Mischak, and then turn around and defend their beliefs by citing physical evidence (and incorrect evidence at that) for a young earth?*

Today's creationists claim that their religious convictions are also scientifically provable. *This perversion of both science and religion makes the creationist movement a danger to free thought and education. The readers of the SKEPTICAL INQUIRER need to guard against this fallacious reasoning as much as against unscientific evidence purporting to prove spoon-bending, astrology, and other claims of paranormal phenomena. Revealed truth may supersede the evidence of our minds and senses, but the two should never be confused with each other.*

Astronomy and creationism—II

Science cannot prove or disprove the existence of God. But science has demonstrated that, if there is a God, he either triggered the Big Bang (or whatever) and then let the universe unfold in the evident fashion, or he created the whole thing a short while ago, complete with the built-in appearance of having evolved over billions of years. Hence, the possibility cannot be ruled out that we were placed on earth by a dispassionate observer or a practical joker, neither of which can be expected to care whether we worship him or not.

In any case, if there is a creator, the question still remains who is the creator of the Creator, and who is his creator, and who his, et cetera ad infinitum.

In the final analysis, creationism

solves nothing. Evolution theory sensibly tries to explain only what lends itself to explanation.

J. E. F. DeWiel
Calgary, Alberta

As a recent subscriber, to say I am most pleased would be an understatement.

The news articles in your Fall 1982 issue regarding "scientific creationism" were of particular interest. The remark creationists use most often is that evolution is "only a theory," thereby misusing the scientific meaning of "theory." I was pleased to see that (page 7) the creationists have finally begun to recognize that the heliocentric planetary system is "unbiblical" and that the geocentric view is the only tenable biblical system.

Many (if not most) scientists outside of biology have not recognized that "scientific creationism" negates not only biology, but astronomy (see the preceding article), anthropology, paleontology, archaeology, physics, comparative anatomy, and a large portion of chemistry.

To point out the pseudo-intellectual nature of such superstitious nonsense is a noble enterprise, and I support such efforts fully.

William C. Zeek
New York, N.Y.

Local CSICOP-type groups

Let me state from the outset that *SI* is, without exception, *the* most outstanding publication I have ever subscribed to. It serves a need not properly addressed by others.

Yet even though CSICOP's influence extends beyond the readers of it's journal, this influence extends primarily to professionals and a few interested lay people. This is an important first step, but one that ignores the immediacy of the problem facing the average skeptic in a world of countless others who prefer to believe in fantasy.

It is insufficient to demonstrate what ought to be without answering the question. "How do you get there from here?" Sadly, CSICOP and it's journal have, perhaps unintentionally, begged this question. Undoubtedly, and to it's critics' delight, CSICOP's current efforts can easily be likened to ivory-tower science. In dispensing high wisdom, critical reasoning, and advocating skepticism among the masses, CSICOP has failed the latter. Given its limited resources and irresponsible silence from the media this is not entirely CSICOP's fault, but it is true nonetheless.

I believe Kendrick Frazier came closest to realizing the solution to this dilemma when he implored his readers to participate in the process of inquiry and evaluation (*SI*, Summer 1982). Unfortunately, he fell just short of the answer by suggesting individual letter-writing rather than *the establishment of local CSICOP-type organizations.* He wrote, "Join us in our task of presenting reliable information about claims on the paranormal borderlands of science and . . . help us get [it]." To this I can only reply: Help us help you! By encouraging the establishment of satellite groups complementary to its own efforts, CSICOP would immeasurably aid its own cause. Such groups could prove instrumental in providing CSICOP with a reliable source of information as well as in enfranchising greater numbers of interested others. Furthermore, grass-roots skeptics could tackle projects difficult for a larger organization. For instance, by enlisting the aid of sympathetic professional magicians, a smaller group could provide Randi-style debunkings in community high schools and colleges.

I, for one, would be more than eager to participate in a worthwhile group devoted to the cause of evidential enlightenment. I can hardly think of an effort I would take more seriously, or an honor I would hold greater, than to be a dues-paying member of my local

CSICOP chapter. I conclude that many others feel the same.

Edward Schnaper
Randallstown, Md.

We are pleased that at least two local CSICOP-type groups have now been formed—one in the San Francisco Bay area and the other in Austin, Texas. We encourage the formation of others (see News and Comment, p. 13).—ED.

Excessive equivocation

After reading James Randi's "Clearing the Air about Psi" (*SI*, Fall 1982), I decided to write regarding excessive verbal hedging by skeptics of paranormal phenomena. Specifically, Randi was complaining about being misquoted because someone had described him as ruling out the possibility of certain phenomena when he had merely stated that they had a probability that would "approach zero *very* closely." Well, excuse me. Randi, and a number of others, such as Ray Hyman, too often hedge when expressing their view of these purported phenomena.

By profession I am a lawyer, and I think I can say that in the everyday affairs of men, as well as in the courtroom, people are comfortable in making statements that are absolute in form even when the probabilities are less than certain. Most people will say something is so when it has a 90 percent chance of being so; likewise, most will say it is not so when it has less than a 10 percent chance of being so. We speak our views and make our decisions with the probabilities implied and nearly everybody understands it in that context. In any event, it certainly is not necessary to overqualify a statement where the probabilities are very close to one or zero.

Such hedging may be more precise in the literal sense, but the choice of language is often inappropriate for the occasion and may be counterproductive in that it conveys an ambiguous message. Overqualification is often interpreted as timidity in taking a stand; if so, the behavior only serves to weaken the strength of the skeptical position by creating the impression that the speaker is more interested in providing a convenient defense to being proved wrong or even that the speaker actually entertains serious doubts about his own arguments.

I probably have overstated my criticism. Randi equivocates less than most skeptics of paranormal phenomena; after all, there are few who will stick out their necks to the tune of $10,000. Perhaps my disappointment in this instance is caused by Randi's usually straightforward approach. However, skeptics should carefully avoid placing themselves in an apologist position. Nor should we deny ourselves the pleasure of a good horse laugh now and then. The worst that can happen is that we would be proved wrong.

Michael A. Corn
Assistant to the President
for Legal Affairs
Portland State University
Portland, Ore.

Don't measure up?

The back cover of the SKEPTICAL INQUIRER describes the work of CSICOP in such terms as "critically examining claims," "carefully examine such claims," etc. A couple of the items under "News and Comment" (Fall 1982) hardly measure up to that standard.

On page 5, Oberg is reported as stating that "other UFO cases . . . may also be accompanied by such unrecognizably false clues that need never be explained—indeed, they must be thrown in the trash can before any explanation is attempted." In other words, some aspects of reports are to be rejected on a priori grounds because

they are misleading, albeit one cannot recognize that they are misleading!

On page 7, Robert Sheaffer writes that "a recent article in a leading creationist publication" shows something. Surely your journal recognizes the necessity of giving chapter and verse for citations; after all, you do not wish your readers simply to accept whatever is written in the SKEPTICAL INQUIRER, you want them to be able to go to the original documents. The worst sort of pseudoscientific writing is full of untraceable and unverifiable statements similar to the one I have just quoted. I do not expect to find that sort of thing in your journal.

Henry H. Bauer
Dean, College of Arts and Sciences
Virginia Polytechnic Inst. and State Univ.
Blacksburg, Va.

Robert Sheaffer responds:
The source was the Bible-Science Newsletter, *March 1981, p. 3.*

James Oberg responds:
It is unreasonable to insist that an explanation for an event be totally consistent with all eyewitness testimony. Any visit to a courtroom should prove this. The recognition that such testimony is frequently distorted (innocently or deliberately) or that the original perception may have been inaccurate to begin with is the only way in which the law can reach verdicts on conflicting claims. In court, or in UFO cases, this is not an arbitrary a priori *process—it is a best guess based on our experience and judgment as investigators. But such unresolved testimony cannot stand in the way of reasonable explanations and a "best guess' at the truth.*

It's all Greek

Dr. Albert Kaplan (*SI*, Fall 1982) loves Randi but thinks "his English needs a bit of polishing." I see nothing wrong with Randi's English, but if Dr. Kaplan is going to use Greek, he should know there is no such thing as "a kudo." *Kudos* (pronounced KYOOdoss or KOOdoss) is singular; it means glory, fame, renown, praise.

Richard de Mille
Santa Barbara, Calif.

The Shroud of Turin

Considering the statement in the SKEPTICAL INQUIRER (Spring 1982) that "We wanted to wait until the work was completed and the results and claims were on record. That is now the case," I was surprised to find three articles purporting to evaluate shroud research to date! While I have some fundamental disagreements with the facts and interpretations of Mueller and McCrone, I have essentially no dispute with them. Schafersman's article is quite different.

We are there treated to a series of confusions, misstatements, and factual distortions descending into ridicule, innuendo, and assorted putdowns. He believes STURP's research is suspect because, as humans, STURP cannot possibly be objective. But Schafersman's own anti-shroud stance shows through—he is his own best illustration when he carries this assumption about subjectivity to the other extreme. Of course, as humans we cannot escape subjectivity. Does that mean we should not try?

Schafersman is a graduate student at Rice University and by his own admission is a specialist in micropaleontology. By writing in the SKEPTICAL INQUIRER I assume Schafersman wishes us to believe he is a skeptic. He draws from many areas of scientific interest to "critique" shroud research. Against their claims and wishes he forces STURP into some bold theological conclusions which he promptly rejects—and, conversely, accepts without question Joe Nickell's conclusions as gospel! Clearly, Schafersman is neither wholly skeptical nor scientific in

his approach to the issues surrounding shroud research.

I would suggest that any broad attempt at evaluation is not only premature but impossible so long as important data remain unpublished. As of this writing there are at least seven articles that STURP has yet to publish. Nor has Joe Nickell released the full details of his artist hypothesis.

This lack of scientific information and objective evaluation has caused some problems. For example, Schafersman writes off Frei's work on pollen by stating, "Most STURP scientists doubt Frei's work and discount his results" (p. 40). Until recently it has remained puzzling why one scientist could find pollen on sticky tape samples and another group could not. The Americans used a device that limited the pounds-per-square-inch pressure on the shroud and forced each tape sample to be removed straight upward. Frei, in a letter to me, confirmed that microscopically he actually observed pollen between the threads of the shroud in 1973 (see also *Shroud Spectrum International*, 1/3 [June 1982]). He requested and obtained permission to use a dime-store tape-dispenser to take samples. But he emphasizes that he had to move the threads *laterally* so that the pollen grains were thrust up onto the tape. He found pollen on at least one-third of his tape samples. Giovanni Riggi, of the Italian branch of STURP, has confirmed finding pollen in abundance using a small vacuum sampler.

Schafersman plays many roles outside his field in his attempt to discredit shroud research. He plays the part of human anatomist when he says that the image's right forearm and fingers on the right hand are abnormally extended (p. 44). He is not aware that STURP believes this is a natural and expected distortion due to cloth-to-body drape. And by pitting Bucklin against Barbet he tries to place Stevenson and Habermas in an embarrassing position (p. 55). But he betrays his anatomical ignorance when he fails to realize that there is no discrepancy between an expanded ribcage, drawn in hollow beneath it, and a distended lower abdomen. For this he may turn to Barbet's own statement in *A Doctor at Calvary* (p. 86).

He plays the role of a hematologist when he says that "the man on the shroud has a curious resistance to coagulation" (p. 45). Yet two Italian doctors have studied the blood flows across the back and see evidence of coagulation where the micropaleontologist sees none!

He is on equally shaky ground when he tackles the archaeological and theological aspects. He seems totally unaware that a crucified victim from the first century A.D. was discovered in Israel in 1968 with clear evidence of nails. He is oblivious of new and important information from the Dead Sea which bears on crucifixion and on the interpretation of John's Gospel. He has overlooked the evidence in Luke 24:40 and Rev. 1:7 in an attempt to prove that "John is trying awfully hard to fulfill the Scriptures . . ." (p. 46). Schafersman has failed to come to grips with ancient Jewish hermeneutics. Jewish boys were raised on Scripture, which they could recite orally. That many ancient Jews commonly found their understanding of current events illuminated by the Scriptures is proven by materials discovered at Qumran. The Scriptures were accepted implicitly. One did not use the present to prove Scripture; one used the Scriptures to understand the present. Schafersman's approach is, thus, an anachronistic one: a modern construct superimposed on the past.

This past July, in Chicago, Schafersman publicly labeled STURP "pseudo-scientists." In my opinion, STURP has made a better case for the shroud as a burial cloth than he has against them!

Paul C. Maloney, Director
Ancient Near Eastern Researches
Quakertown, Pa.

Steven Schafersman responds:

I appreciate the opportunity to reply to

Mr. Maloney. I admire the serenity of an individual who has no dispute with persons with whom he has fundamental disagreements. I am therefore curious why he wishes to dispute me, and especially in such vituperative terms. Since fundamental disagreements are not the issue (and surely there are plenty of these!), I can only guess at what the hidden reason may be. Because I presented the abundant evidence that the shroud of Turin is an artifact, and thoroughly refuted the claims that it is a genuine burial shroud, I suppose it was inevitable that my anti-shroud stance would show through. I assume that Mr. Maloney would prefer that I not take sides on this issue; perhaps this would be "objective" in his eyes. My point about objectivity was that humans are subjective but scientists can approach objectivity by accepting all valid evidence and legitimate criticism, by adandoning closely held but clearly incorrect hypotheses, and by repeating critical observations and experiments. The STURP scientists have not done these things.

STURP's "bold theological conclusions," which Mr. Maloney claims I force them into, were drawn entirely from STURP's own written and spoken statements; I had nowhere else to get such conclusions, and I certainly do reject them—because they are unscientific. Max Frei's pollen results are incredible for many reasons; I treat this topic in detail in The Microscope *in early 1983. No other scientist, with two additional sets of sticky tapes, has seen the pollen that Frei claims to have found, and no one save Frei has reported observing pollen between the threads of the shroud. I was indeed unaware that STURP is now explaining the unnaturally elongated right forearm and fingers as "natural and expected distortion due to cloth-to-body drape." STURP has been saying for years that the figure is anatomically perfect in every detail and shows no distortion. And, if we're now going to have some cloth-to-body distortion, why none on the face? Mr. Maloney is correct about the distinction Dr. Barbet made between the upper and lower abdomen, but both Dr. Bucklin and Stevenson and Habermas failed to make the same distinction. So, although I stand corrected, I don't blame myself; I wish Mr. Maloney wouldn't blame me, either. Two Italian doctors see evidence of coagulation on the back! Why nowhere else? I respect the medical opinion of Dr. Michael Baden, chief medical examiner of New York City, who sees no coagulation anywhere on the image. Contrary to Mr. Maloney's contention, I am familiar with the archaeological evidence and the biblical passages he cites; but none of it serves to discredit my conclusions. The only evidence that Jesus was crucified with nails is in John's Gospel. For the record, I am pleased to clarify that all four evangelists, not just John, wrote their semi-fictional books so as to fulfill Scripture.*

In Chicago, I publicly labeled a few *STURP members as pseudoscientists; the majority of STURP's members are good, if, in my view, somewhat misguided, scientists. Readers can find my remarks on this subject in* The Microscope *mentioned above.*

No trace of Sasquatch

Since there have been several references to Sasquatch in recent issues of the SKEPTICAL INQUIRER, I thought perhaps you might like to have some comments on the subject from a professional biologist.

Since 1964, my students and I have studied bats in the lava-tube caves at Mt. St. Helens and in the talus caves on the Chuckanut Mountain near Bellingham, Washington. Both areas have been reported to be inhabited by Sasquatch. The Chuckanut site has not been mentioned very often. However, about 10 years ago, a man came to Bellingham and reported that the trails in and near the Chuckanut Caves were made by a colony of Sasquatch. We knew the trails quite well. They had all been made by people who had worked

with me over the years. I never did find out why the individual was spreading the story but I assumed he just wanted some attention. As far as I could tell he was not one of those who had previously been interested in Sasquatch.

The caves at Mt. St. Helens are in a lava flow on the lower, southern slopes a few miles east of McBride Lake and a few more miles south of Ape Canyon. McBride Lake is supposed to be one of the better locations to look for Sasquatch. Ape Cave is one of about 10 large caves in the lava flow. There are a couple of dozen smaller named caves that I know of and numerous small holes. In the last 18 years, we have taken over 50 trips to the area, sometimes for several days at a time. Some of these trips included McBride Lake. I have spent the night on the mountain alone or with others a couple of dozen times. We have been there at all seasons of the year, including when the snow was 10-feet deep. Some of our studies involved traveling and working at night both in the summer and in the winter.

For 20 months, the lava-flow area was in the Red Zone, which was closed to entry in connection with the recent eruptive activity of Mt. St. Helens. As scientists, we were allowed into the area on 20 different days. Very few others were working on that side. The ashfall and the mudflows produced by the eruptions made an ideal surface for recording tracks. Thus, if Sasquatch existed in the area and moved around at all, we should have seen some sign of them. However, to date I have not seen or heard anything that could be mistaken for a Sasquatch. The only tracks have been those of humans and of such animals as elk, deer, and bear.

The closest thing to a Sasquatch track that I ever saw was before the eruption. One spring we came across a trail of tracks in some heavy snow a couple of feet deep. The tracks were spaced like a humans would be but were several inches longer and wider than a fresh bootprint. I felt that they represented human tracks that had enlarged during melting. I have seen the same effect in the snow in my own backyard.

There is no evidence in any of the caves I have checked that human-sized animals have ever lived there.

It is relatively uncommon to find bones of animals in the wild. Perhaps Mt. St. Helens is a more likely place than most in that regard. The area is heavily hunted for both elk and deer. Undoubtedly, many animals are wounded but escape only to die later. There are steep cliffs and numerous cracks and pits in the lava. Animals can easily fall and be killed or trapped in such places. Often the carcass is not readily available to carrion feeders. Thus the skeletal remains may be more likely to be found. I have found about a dozen partial skeletons. None were questionable. All the larger bones are obviously of elk and deer.

Thus, even though I cannot prove that there are no Sasquatch in those two areas, I certainly can provide a lot of data on where and when they were not present. I think it is obvious that I do not believe Sasquatch exist in those places.

Clyde M. Senger
Professor of Biology
Western Washington University
Bellingham, Wash.

Naive view of medical advances

I do a great deal of popular medical writing, and it's easy to be cynical. Sometimes I'm convinced that most Americans prefer magic potions and witch-doctory when they are ill. They go to medical doctors not because they realize the truth of scientific medicine but because M.D.'s are the socially acceptable healers.

The magazines I write for (all women's magazines; there is essentially no market for health articles in publications read by men) are eager to let their readers know about legitimate medical advances. But they are just as happy to tell them about nutrition fads, miracle diets, magical cures, and amazing

discoveries by humble geniuses ignored by the establishment. Skepticism is verboten.

It's frustrating. It's also one reason I find your magazine so refreshing. It gives me the feeling of belonging to a sensible, clear-thinking (though unfortunately small) elite who despise cant, humbuggery, and bogus science.

What a shock to discover that one of your readers—worse, a subscriber—holds one of the most naive theories about the nature of medical advances. It's very popular, but to find it expressed by a fan of the SKEPTICAL INQUIRER is profoundly disturbing.

I refer to the letter by Pellie Vreeland in the Fall 1982 issue, stating that we should all have more skepticism about claims by the "medical establishment (including scientific spokesmen) that you need not apply . . . to any other of the main institutions of science." Elsewhere the letter gives the reason why the medical establishment denounces important advances and is not to be trusted—the pressures of all that wealth ("billions and billions of dollars").

The belief that powerful organizations conspire to suppress the truth is universal among groups CSICOP was formed to fight (i.e., the Air Force hiding proof that flying saucers are extraterrestrial "evolutionists" banding together to pooh-pooh evidence for creation, etc). Probably the most common fantasy along these lines is that major medical advances are often denounced by the narrow-minded medical establishment until, after a long struggle by a few courageous scientists, their truth is accepted. Enthusiasts for laetrile, DMSO, and vitamin B-15 are convinced that their pet miracle is undergoing the usual trial by fire.

Most SKEPTICAL INQUIRER readers realize this is nonsense, but what's surprising is how easy it is to refute. You can do it with a short request: name one.

I routinely challenge critics of mainstream medicine to name one important advance in this century that was denounced by medical organizations and only triumphed because a few stubborn pioneers defied them. By an important advance I mean a discovery like penicillin, insulin, cardiac pacemakers, polio vaccine, artificial joints, open-heart surgery, kidney dialysis, etc. So far no one has succeeded.

Maybe Pellie Vreeland will be the first. Vreeland writes: "The fact that the medical establishment says something is quackery doesn't begin to mean that it is."

Is that so? Give us examples.

Michael D. Oppenheim
Los Angeles, Calif.

The press and the paranormal

I noted with interest the short article on "Superstition and the Press" in the Summer 1982 issue. My position at the university often brings me into contact with journalism students who have told me they are taught specifically that the entertainment value of news stories is the overriding concern for the writer. If facts must be sacrificed, so be it. (The University of Missouri-Columbia School of Journalism considers itself the number one school in the country.) But I find that students are very uncritical in both their acceptance of information (they cannot discern between good science and bad science) and their interpretation of and re-expression of information (what in print may be attributed to oneself may bear little resemblance to what one actually said). This last point is a particular annoyance as it is a not uncommon practice for reporters to phrase part of their own writing as if it were a direct quote. While one can attempt to control this problem by requiring approval of the final draft of a news article, this is not foolproof.

Several years ago a student wrote an article on the phenomena of the Star of Bethlehem on the basis of an interview with me. The standard idea of a

planetary conjunction ca. 7 B.C. and its astrological significance in that era were discussed. And since the student brought up the Jupiter effect I described why it was nonsense. This became part of the student's piece. The actual article, approved by me, was a reasonable exposition meant only for local use. What was unforeseen was that UPI would pick up the article and rewrite it to the effect that I was predicting the reappearance of the Christmas star in 1982. This, of course, was published worldwide.

Last summer there was a report of the spontaneous combustion of a human being in Chicago. We saw the original story while in Europe, in the *International Herald Tribune*, which noted that eight prior cases of this phenomenon had been authenticated. Upon returning home, a review of our accumulated local and St. Louis papers revealed that both had carried the original report. Of these three papers, however, *only one*, the *Columbian Daily Tribune*, carried the follow-up that revealed that the victim was already dead when deliberately set on fire.

C. J. Peterson
Department of Physics and Astronomy
University of Missouri
Columbia, Mo.

'Out of body' experiences

I have just, belatedly, been reading the Spring and Summer issues of *SI*, and would like to make an observation on "out of body" experiences.

When I was a kid, thanks to a gardener who put raw sewage on the lettuces in our garden, I contracted violent dysentery. It left me with an agonizing condition called spastic colon. For years I had bouts of such extreme pain that I used to have to escape it by imagining that I was looking down on my body from outside. I can still call up the sight of myself viewed, as it were, from that corner of the ceiling. This was in no way a conscious process, by the way; it was a sort of defense mechanism, and purely reflexive.

No more do I regard it as supernatural. To me it was and is purely an example of how efficient the imagination can be, especially in the case of a child. But one can well see how, to someone not raised in a rationalist and skeptical environment, it might appear to be a "real" phenomenon. I wonder whether anyone has ever attempted to establish, in the backgrounds of people who claim to be able to attain so-called "astral projection," a history of acute pain in childhood. I'm certain I can't have been the only small boy who found escape from immediate suffering.

John Brunner
South Petherton
Somerset, U.K.

A "great" mistake

In my review of Andrew Neher's Psychology of Transcendence (*SI*, Summer 1982), I mentioned that to some people the only danger in occultism is that they might turn away from rational thinking. I had said in my original draft of the review that "Neher sees a greater danger," in that occult beliefs tap into powerful psychological processes that lie outside of consciousness.

Due to a typographical error in my final draft, the word "greater" was truncated to "great." This wording gives the unfortunate impression that Professor Neher is arguing that occult beliefs pose some sort of menace. This is quite incorrect; for, while he does write of the potential dangers due to the powerful psychological forces that can be brought into play, he at no time suggests that there is any cause for alarm.

James E. Alcock
Glendon College
York University
Toronto, Ontario, Canada

The Project Alpha Experiment: A Preview

For several years, a number of observers of parapsychology have been privy to a secret experiment, code-named Project Alpha. It was being carried out to test the ability of researchers in a well-funded university parapsychology laboratory to detect obvious magicians' trickery in their research subjects. The existence and results of the experiment were publicly announced on January 28, 1983 (first in a mailing by magician James Randi to parapsychologists, then in a *Discover* magazine news conference in New York City, in a 15-minute segment in the NBC television special "Magic or Miracle" on February 8, and in an article in the March *Discover* magazine). The disclosures have rocked the parapsychological community. Some have said the experiment sets back parapsychology a hundred years; others have praised it as a long-needed and welcome effort of housecleaning.

The experiment was designed by Randi, and put into operation in 1979 following the McDonnell Foundation's award of $500,000 to Washington University in St. Louis to establish a laboratory of psychical research. He felt this was an opportunity to test the claims of parapsychologists that they could do credible research if only they had adequate funding and that they did not require outside help to know whether or not they were being deceived by sleight-of-hand.

Two young conjurors posing as psychics, Steve Shaw and Michael Edwards, were introduced into the laboratory and were accepted and tested there. Before tests began, Randi sent the laboratory's lead researcher, physicist Peter R. Phillips, a list of eleven things to watch for in testing psychic claims of psychokinetic metal bending. These were warnings to adhere to rigorous controls and not to let the two subjects modify the protocols to their own liking. Randi later supplied specific details concerning the methods the two men were using to do their "telekinesis" and other "ESP" tricks. "I feared that these warnings would be ignored," Randi says. "I was right."

All of Project Alpha's hypotheses were firmly established. In spite of the warnings, the Washington University researchers continually accepted gross breaches of research protocol. They never asked any basic questions that would have resulted in exposure. (Randi had instructed Shaw and Edwards to disclose their real role as magicians if ever directly asked; they were not.)

The subjects were regularly able to fool the researchers into believing they had accomplished feats by apparently paranormal means. Progress reports and updates from the laboratory contained statements such as: "We do believe we have witnessed PKMB [psychokinetic metal-bending]," and ". . . a great variety of PK events took place."

In articles in future issues of the SKEPTICAL INQUIRER, James Randi will outline Project Alpha's design and results in full detail and explore their implications for parapsychology.

—The Editor

The Committee for the Scientific Investigation of Claims of the Paranormal

Paul Kurtz, Chairman

Scientific and Technical Consultants

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- Maintains a network of people interested in critically examining claims of the paranormal.
- Prepares bibliographies of published materials that carefully examine such claims.
- Encourages and commissions research by objective and impartial inquirers in areas where it is needed.
- Convenes conferences and meetings.
- Publishes articles, monographs, and books that examine claims of the paranormal.
- Does not reject claims on a priori grounds, antecedent to inquiry, but rather examines them objectively and carefully.

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